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SECTION OF GEOLOGY AND MINERALOGY

THE ORIGIN OF RED BEDS

By PAUL D. KRYNINE*

Introduction. Red colored sediments have always attracted the attention of geologists. A venerable opinion of long standing connects red beds with aridity, thus investing them with an important paleoclimatic significance. Red beds are rather extensive, forming, for instance, close to 15 per cent of the Paleozoic section in the Appalachian region. The red bed problem has been extremely controversial, with marked differences of opinion, possibly due to the fact that the term "red bed" is a catchall for many sedimentary types produced under different conditions, the only common feature of them being the red color.

Source of Red Color. Red color in sediments is produced either by the presence of red minerals such as orthoclase or, more commonly, by the presence of red ferric oxide pigment occurring either as anhydrous hematite or as red limonite (turgite), in amounts as small as two or three per cent of the rock.

Red beds in the field show considerable variation of color. The amount of actual red rock within a markedly red bed series may be as low as 35 per cent. Many red beds do not carry more than ±50 per cent of actually red colored rocks. The balance consists of drab, gray, green, brown or even black colored rock units. The two basic shades of red are a deeper cherry red (a typical Triassic and Paleozoic color) and an orange red (generally more characteristic of the Permian). Red iron oxide pigment produces shades of purple when mixed with such green minerals as chlorite or glauconite.

Classification of Red Beds. Red ferric oxide pigment can permeate a sediment in a variety of ways. As a whole its presence indicates a prevalence of oxidation over reduction, a phenomenon that takes place most easily above the water table, thus explaining the great predominance of continental red beds over marine red beds. Otherwise, ferric oxide can enter almost any sediment and, hence, practically every normal sedimentary type may occur as a red bed facies. However, in certain sediments, this red bed facies is much more prominent and frequent than in others.

Like all normal sediments, red beds can be divided into detrital and chemical. In *detrital* red beds, the red pigment entered the basin of deposition as a solid detritus, generally in the form of hematitic red clay. In *chemical* red beds, the red pigment is precipitated from solution within the basin of deposition. Finally, the red pigment may develop authigenically in the sediment itself, after deposition, through the oxidation of ferrous oxide. A pragmatic fourfold classification (not entirely logical, since it combines certain elements of all three preceding modes of entry) results in four basic types of red beds (FIGURE 1).

(1) *Red beds produced from red soils or primary red beds.* The red pigment is developed by weathering in the source area of the future sediment, at the expense of iron-bearing minerals (silicates or carbonates) and is incorporated directly into the resulting sediment. This can be done in three ways:

(a) After erosion and transport, producing Primary Detrital red beds.

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* Professor of Petrology and Sedimentation, The Pennsylvania State College, State College, Pa. This paper, illustrated with lantern slides, was presented at the meeting of the Section on December 6, 1948.

(b) By *local* reworking of the regolith, producing Primary Reworked red beds.
(c) By lithification of the regolith without hardly any reworking at all, producing Primary Residual red beds.

(2) *Red beds produced from non-red detritus by oxidation within the sediment itself.*
(a) Immediately after deposition and before burial, producing Post Depositional red beds.
(b) After burial, emergence and deep sub-surface oxidation producing Post Diagenetic red beds.

(3) *Red beds produced through the reworking of older red beds* or Second Cycle, or Secondary red beds (generally eolian or glacial, less commonly aqueous).

(4) *Red beds produced chemically by precipitation from a solution within the basin of sedimentation.*
(a) Under marine conditions.
(b) Under fresh water conditions.
(c) Authigenically and intrastratally by infiltration within the sediment.

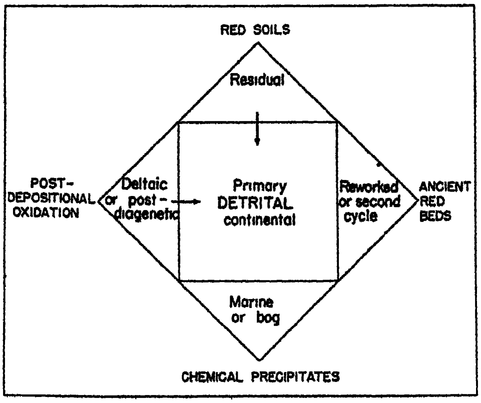

Figure 1. Classification of red beds, based on genesis and provenance of ferric oxide pigment.

Primary Red Beds: Detrital, Reworked and Residual

Significance of red soils. Primary red beds originate from red soils, thus implying a certain geomorphic and climatic picture in the source area (FIGURE 2). The required physiography consists of a reasonably well drained region, with a slope sufficient to maintain the soils in the zone of oxidation above the water table (slope possibly as low as several feet per mile). The climate necessary to produce red soils rather than brown or yellowish soils (meaning hematite and turgite rather than ordinary limonite) is as follows:

EXCESS OF OXYGEN =
WELL DRAINED REGION

PRODUCES RED OR YELLOW COLOR

RED COLOR APPEARS
AT 60° F. TEMPERATURE
and
>40" RAINFALL ON SILICATES
or
>25" RAINFALL ON CARBONATES

Figure 2. Physiography and climate necessary for the production of red soils.

(1) For the red weathering of *silicates*, mean annual temperature above 60°F. and over 40″ of annual rainfall.
(2) For red weathering of carbonates ("terra-rossa"), 55–60°F. of mean annual temperature and over 25″ of annual rainfall.

At least 95 per cent of present day red soils are formed above 60°F. and 40″. Most of the 5 per cent of so-called non-humid red soils were not formed at the present time, but were inherited from the pluvial Pleistocene period. The so-called "red sands of the desert" are mostly fiction in so far as the Recent goes, and are practically always produced by the reworking of the pluvial Pleistocene red soils or of ancient red sediments.

The intense weathering processes that produce limonite and hematite frequently, not only as red disseminated material, but also as ferruginous concretions, lead at the same time to the production of other stable products of weathering, such as hydromicas and particularly kaolinite and gibbsite.

Preservation of red color. The hematitic red clay formed in the regions of red soils is transported into basins of deposition and, if the red color is preserved, a red sediment follows. Preservation of red color implies only one fact, namely, the predominance of oxidation over reduction, a condition that can take place *under any climatic condition.* The controlling factor is a physiography which makes it possible for free oxygen to come in contact simultaneously with the ferric oxide and whatever organic matter incorporated in the sediment. If these conditions are satisfied, *i.e.* if deposition takes place above the water table, which is a feature of practically every well-drained interfluve or subaerial portion of a delta, then the chemical reaction involved is as follows (FIGURE 3):

$$Fe_2O_3 + C + O_2 = Fe_2O_3 + CO_2.$$

Thus, the organic matter gets oxidized and destroyed by an excess of oxygen without ever having had a chance to reduce the ferric oxide.

If, however, deposition takes place under the water table, meaning swamps, lakes, channel deposits, etc. then instead of the excess of oxygen there may be a deficiency and conversely a possible excess of organic matter, and the equation will be as follows:

$$2Fe_2O_3 + 2C = 4FeO + C + CO_2.$$

Thus, red ferric oxide is reduced to colorless ferrous oxide and the red color is lost. At this stage, since ferrous oxide is soluble, a certain amount of iron will be removed, or transferred to another part of the formation, where it may be re-precipitated as an iron carbonate.

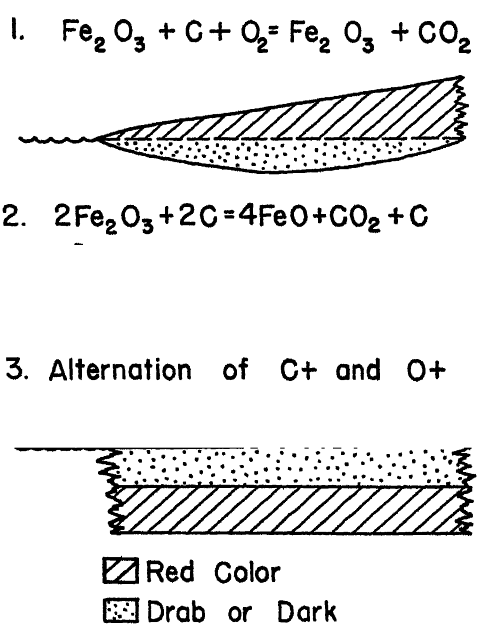
1. $Fe_2O_3 + C + O_2 = Fe_2O_3 + CO_2$
2. $2Fe_2O_3 + 2C = 4FeO + CO_2 + C$
3. Alternation of C+ and O+
▨ Red Color
⬚ Drab or Dark

Figure 3. The oxidation-reduction reaction within the basin of deposition.

In reality, the oxidation-reduction relationships are much more complex and relativistic than indicated by these schematic equations. An important factor is sufficient time for oxidation to take place (a tectonic factor). Also, oxidation is greatly speeded up at higher temperatures and in the presence of much rainfall. Finally, this inorganic oxidation scheme can be translated into terms of destruction of organic matter by aerobic bacteria. Since, however, bacterial activity obeys Vant Hoff's law, the inorganic time-temperature-moisture relationship remains unchanged. As a whole, the geomorphic and climatic conditions best suited for the formation of red soils are also the best for the preservation of red color in the resulting sediment.

Under normal tectonic subsidence, the surface of a basin of deposition may oscillate slightly, changing from a drained slope of deposition above the water table (one to two feet per mile are sufficient to maintain adequate drainage) into a submerged or semi-submerged basin. The layer formed above the water table may be red, but, if the basin then passes from the drained (open) to the undrained (closed) stage, the next layer will be colorless, green, gray or even black. However, the underlying (previous) red layer is not reduced, since the organic matter in it was already destroyed by previous oxidation (infiltration of reducing solutions from above is possible but rare). Another typical continental setup for the alternation of red and non-red layers occurs through the simultaneous existence of channel deposits, in which reduction takes place, and of interfluves or flood plain deposits in which oxidation predominates (FIGURE 4). Channel deposits are normally medium clastic sands, whereas the flood plain deposits are clayey silt. The lateral migration of these physiographic loci produces alternating gray sandstone and red shale or siltstone layers.

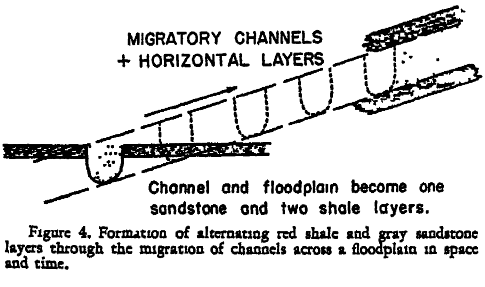
MIGRATORY CHANNELS
+ HORIZONTAL LAYERS
Channel and floodplain become one sandstone and two shale layers.

Figure 4. Formation of alternating red shale and gray sandstone layers through the migration of channels across a floodplain in space and time.

It can be seen, thus, that an arid climate is not required for the formation or preservation of red detritus. The concept of *absolute* oxidation (no organic matter, and hence a desert) or of *absolute* reduction (organic matter present and hence not a desert) is fallacious and must be replaced by the relative concept of fluctuating oxidation or reduction periods of adequate length due to a *relative* excess or deficiency of oxygen as against available organic material. The control is, hence, not so much climatic as physiographic, meaning tectonic. All available paleoclimatic criteria, fauna and flora, almost uniformly point to the existence of very hot and very *humid climates* during the formation and deposition of most primary detrital red beds.

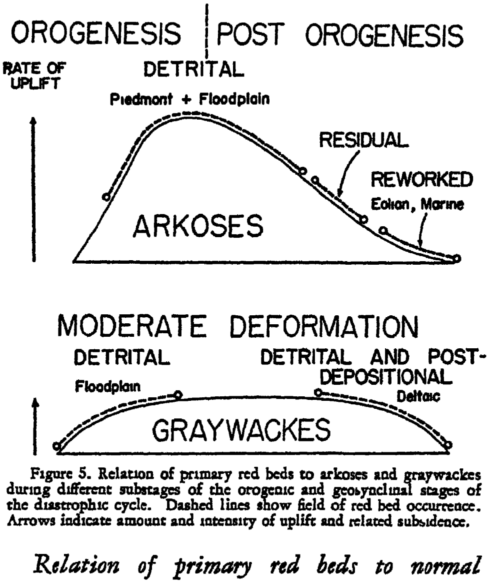

Figure 5. Relation of primary red beds to arkoses and graywackes during different substages of the orogenic and geosynclinal stages of the diastrophic cycle. Dashed lines show field of red bed occurrence. Arrows indicate amount and intensity of uplift and related subsidence.

Relation of primary red beds to normal sediments. Primary red beds normally are continental deposits, generally related to periods of strong orogenic activity, and, hence, to the large scale emergence of continents. Thus, they are normally associated with arkoses. The tectonic activity of the orogenic stage of the diastrophic cycle can be divided into a combined early tectonic and a main tectonic phase on one hand, and a gradually declining late- and post-tectonic phase, on the other (FIGURE 5). These phases are characterized respectively by tectonic arkoses occurring in extremely thick deposits and by post-tectonic or residual arkoses occurring more frequently as small patches and thin broken blankets. Frequently, both of these arkose types are red, thus producing two major red-bed types: namely, the *primary detrital red beds* (generally red tectonic arkoses) and *primary residual red beds* (mostly sedentary arkoses).

Formation of arkosic red beds. Physiographically tectonic arkoses are formed essentially in the piedmont and upper-reaches of river plain systems. Lithologically they are thus characterized by many conglomerates and fanglomerates (since tectonic arkoses are frequently generated by powerful block faulting); by coarse sandstones, by a very small amount of true shales (the so-called arkosic red shales being mostly siltstones or even clayey sandstones). The operative erosional mechanism of an active fault block (or any rapid uplift!) implies two loci of weathering and erosion (FIGURE 6). One is the surface of the interfluves, on which red soils may develop under adequate climatic conditions (over 60°F. and 40″ rainfall). If the climate is tropical humid, the red soil on these interfluves may be held rather solidly by vegetation and, hence, the absolute amount of material eroded from such interfluve is rather small, regardless of the fact that they may cover most of the area of an entire region. The other locus of erosion is the bottom of the very steep V-shaped canyons, which characterize such youthful topography. By undermining of the oversteepened banks, very large masses of fresh bed rock, completely non-weathered, may be eroded. The resulting mixture in a granitic source area will consist of a fresh, hardly weathered granitic detritus, coming from the bottom of the canyon; and hematite bearing, deeply weathered red clay, rich in kaolinite and possibly even carrying gibbsite, coming from the interfluve.

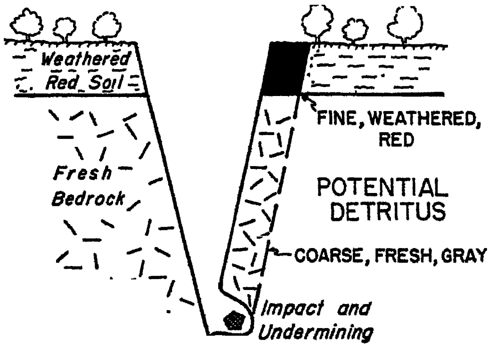

Figure 6. The V-shaped canyon, the basic geomorphic unit for the formation of red arkosic detritus.

In the Triassic of Connecticut, these two petrographic end members, namely fresh granitic detritus and hematitic red clay, have the following average composition:

Granitic Detritus	*Red Clay*
Quartz 58%	Kaolin 60%
Microcline 31%	Gibbsite 6%
Sodic plagioclase 9%	Sericite-illite 12%
Mica 2%	Hematite 20%

The same mineral (microcline for instance) will be fresh and unweathered if it comes from the bottom of the canyon and deeply decayed if it comes from the interfluvial red soil. Such an association in one single specimen is quite diagnostic.

The deposition of this mixture takes place on the piedmont, where an unmixing takes place, with the coarser granitic detritus segregating itself into gray channel sandstones, while most of the red clay forms the red silts and shales of the flood plain (FIGURE 4). Since this unmixing is never perfect, a good bit of the channel deposits are colored red. Under even less favorable sorting a red clayey sandstone may be formed (the "Redstone" of Connecticut).

Examples of primary (mostly detrital) red beds. The Triassic Newark formation of the Eastern United States is a fine example of primary detrital red beds. It is found for a length of 1,200 miles from South Carolina to Nova Scotia, and forms a series of extremely thick, prismatically shaped, faulted troughs of deposition, in which up to 15,000 or more feet of sediments may be present. The Triassic of Connecticut, studied in considerable detail by the writer, contains 52 per cent of actual red beds. The lithological breakdown is: Red sandstones 31 per cent, Red siltstones and shales 21 per cent, gray sandstones 41 per cent, dark shales 6 per cent.

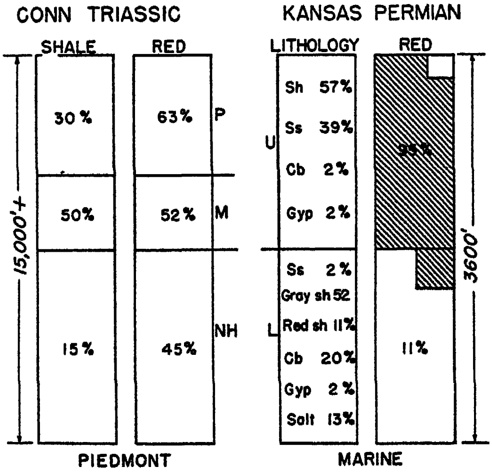

Figure 7. Comparative lithology and color distribution of two primary detrital red beds. The Connecticut Triassic (only shale percent shown, balance is sandstone and conglomerate) is piedmont and the Kansas Permian near shore marine. Triassic formations are: P—Portland, M—Meriden, NH—New Haven; Permian: U—Upper, L—Lower. Cb means carbonates: Data based on field work for the Triassic by P. D. Krynine and for the Permian by Ada Swineford.

During most of Triassic time, the bottom of the trough subsided at high speed, but the surface of deposition was a *well drained* slope of deposition, the sediments thus staying red. During Middle Newark time (Meriden epoch), however, the drainage was impounded by warping and extensive lakes and swamps developed producing dark shales (FIGURE 7).

Similar relationships exist in the Fountain formation of the Front Ranges, the Sespe of California and certain parts of the West Texas Permian. Sometime, sthese tectonic arkoses may stretch farther into the lower reaches of a river system, and even into marine near shore beds as in the Permian of West Texas.

Although the diastrophic cycle and tectonism control all sedimentary processes, nevertheless the actual formation of a sediment takes place within a certain locus of deposition (or environment). The relationships between certain typical red beds and the major classes of depositional loci are shown in FIGURE 8.

Graywackes are related to moderate deformations of the earth's crust and, hence, tend to be deposited typically in the deltas and lower river plains. Thus, the red beds connected with the graywackes are also deltaic or nearly so. Examples of this are many of the detrital red beds of the Paleozoic of the Appalachian region such as the Upper Ordovician Juniata formation, the Silurian Bloomsburg red beds, the Devonian Catskill, the Tertiary Siwalik of Northern India. Although most of the deltaic and flood plain detrital red beds are primary, nevertheless a certain amount of post-depositional exidation may reinforce the original red color.

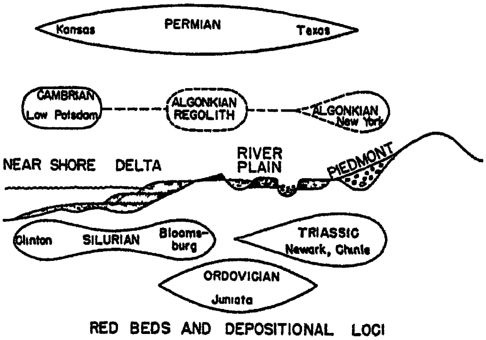

Figure 8. Relation of typical red beds to loci of deposition.

The *quartzites* may also carry detrital red beds. However, since large scale quartzites of the Belt series or Potsdam type are formed under conditions of tectonic quiescence (which corresponds depositionally to gently subsiding peneplanes) the red beds connected with these quartzites are generally of the *reworked* type. This means that the residual red beds developed in arkoses and, specifically, the red regoliths occurring on the peneplanes produced by erosion and planing of an orogenic region, may be incorporated into an advancing quartzitic marine sediment and color it red. The Pre-Cambrian-Potsdam contact north of the Adirondacks shows beautiful remnants of Algonkian (?) primary detrital red beds of the tectonic fanglomerate arkosic type, morphologically non-distinguishable from the Connecticut Triassic. Side by side occur residual red beds of much later age, related to post-Algonkian peneplanation, and both of these are covered unconformably by still later, well rounded, Potsdam quartzites. The red iron oxide pigment from both the fanglomerates and regoliths has been mechanically incorporated into the basal quartzites, coloring them red and producing primary reworked red beds (FIGURE 9).

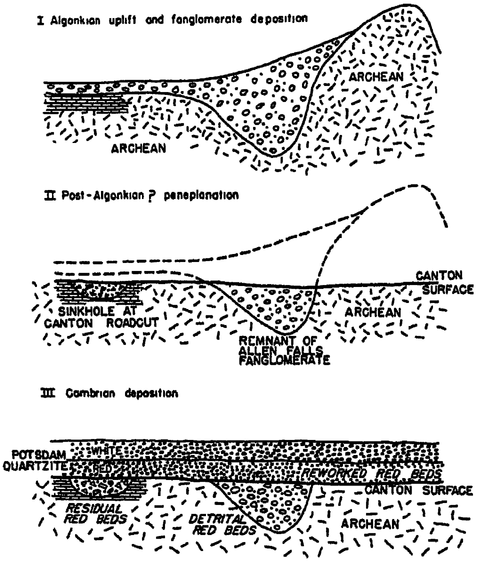

Figure 9. Early and late Algonkian and Cambrian paleogeography of New York State showing tectonic, residual, and reworked red beds, all of them primary.

Post Depositional and Post Diagenetic Red Beds

An iron bearing sediment oxidized after deposition may turn red. This takes place on sub-aerially exposed interfluves of deltas and may change blue and green to red. The submerged part of the delta stays green and, if the movements are oscillatory, an alternation of red and green beds takes place. Such red beds differ from primary detrital red beds in that oxidation has taken place *locally* after deposition and the basic background of the entire series remains either blue or green. The reverse is true in primary detrital variegated red beds, namely the non-red color has been reduced and the basic background of the formation is red to begin with. Post depositional deltaic red beds on a basic background of blue clays exist in the Irrawaddy delta. A sizable portion of ancient deltaic red beds may be of this character, although probably most of them are primary detrital.

When a sediment is brought after burial and emergence into the zone of oxidation (but not of actual surface weathering), its susceptible minerals containing ferrous oxide, such as glauconite, may be oxidized for a depth of 100 feet and more. Such exposed and oxidized sediments are termed post-diagenetic red beds (FIGURE 10).

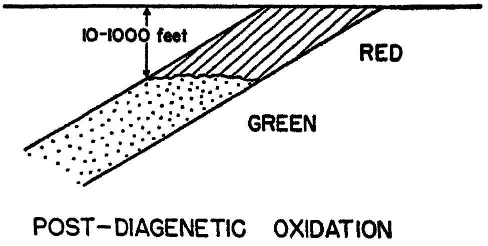

Figure 10. Post-diagenetic red bed formation by subsurface oxidation.

Volumetrically speaking, post-depositional and post-diagenetic red beds are much smaller than the primary ones.

Secondary Red Beds

When a red sediment (not a red soil or regolith) is eroded during a new cycle, its red pigment may form a new red sediment. Naturally, the paleoclimatic significance of such a secondary red bed is nil. Many eolian sediments are of this type, including most of the famous "red sands of the desert" which, in the case of the Northern Sahara red sands, are reworked fluvial Oligocene red sands, and in the Arabian desert are Red Cretaceous sandstones. Many glacial sediments are also red, such as the "pink sand" of Connecticut, derived by glacio-lacustrine reworking of the underlying Triassic; the so-called bloody clays of Bavaria and the red glacial clays of Sweden.

Chemical Red Beds

In all the preceding classes, the red pigment, either actual (primary or secondary) or potential (post-depositional and post-diagenetic) was present as a *solid* within the sediment. In the case of chemical red beds, the red iron oxide pigment is precipitated into the sediment from *solution*. Such precipitation can take place either in a marine or fresh water basin during the formation of a sediment or from circulating solution, intrastratally, after deposition. The vast bulk of chemical red beds are marine and include practically all the iron ore formations of the world, such as the Lake Superior and other Huronian iron ores (Brazil, Russia, Labrador, etc.) and the Silurian Clinton iron ores of the Appalachian region. The marine precipitation mechanism of hematite is somewhat in doubt. Volumetrically, the marine chemical red beds take second place after the primary detrital red bed and, thus, form the second big principal mode of red bed occurrence.

Marine *chemical* red beds are *not* related either to salt or gypsum. The interfingering between evaporites or marine chemical precipitates, and marine *detrital* red beds, has no climatic significanc. On the contrary (in the Permian), as a general rule, when the percentage of salt, gypsum and dolomite goes up, the proportion of red beds goes down (FIGURE 7).

Fresh water red beds form reddish (or rather reddish brown) deposits containing siderite, limonite (including turgite) and similar so-called "bog" iron ores. These are very small volumetrically. Finally, authigenic red beds or rather small red streaks or laminae may form in many subaerially exposed sediments including glacial terraces through the precipitation of limonite from circulating solutions.

Conclusions. Most detrital red beds are formed under warm and moist climatic conditions, possibly in tropical savanna type climates. There is no connection between red color and aridity, the contrary being true. Tectonically red beds are connected mostly with positive movements and frequently are related to strong orogenesis and subsequent peneplanation.

References

The concepts presented in this paper summarize the ideas developed by the writer during his work on Red Beds begun in 1929. The following red beds have been investigated petrographically in *some detail* and those marked with the symbol (fw) have also been studied in the field:

Pleistocene and Recent: Glacial deposits of Connecticut (fw); red soils from Burma; red soils and red sediments from Tabasco (fw).

Tertiary: Siwalik series of N. W. India; Sespe of California (fw).

Upper Mesozoic: Various Jurassic red beds from Utah.

Triassic: Newark series from Connecticut (fw), Massachusetts (fw), New Jersey (fw), Pennsylvania (fw); Chinle formation from several Western States (fw); German Triassic.

Permian: Various Permian Red Beds from West Texas (fw), Oklahoma (fw), and Kansas; German Permian.

Pennsylvanian: Fountain formation of Colorado (fw).

Balance of Paleozoic: All from the Appalachian Region; Mississippian Mauch Chunk (fw); Devonian Catskill (fw); Silurian Bloomsburg (fw), and Clinton (fw); Ordovician Juniata (fw); Cambrian Potsdam (fw).
Pre-Cambrian: Algonkian (?) from New York State (fw); Belt series; Lake Superior iron ores.

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* An excellent summary of the literature on red beds and a discussion of the various views on their origin, prevailing at the beginning of 1948, can be found in the paper by: Van Houten, F. B. 1948. Origin of Red-Banded Early Cenozoic Deposits in Rocky Mountain Region. Bull. Am. Assoc. Petr. Geol **32:** 2083–2126.

SECTION OF BIOLOGY

BARIUM METABOLISM IN HORNETS STUDIED BY MEANS OF RADIOISOTOPES

By V. T. BOWEN*

The experiments reported in this paper were stimulated originally by the discovery, to be reported elsewhere, of hornets of the genera *Vespa* and *Vespula*, as notable accumulators of manganese. Investigation of manganese in these organisms had been prompted by the need for data on other *Hymenoptera*, for comparison to Vinogradov's studies of manganese in ants.[1,2] During discussion of the facts of hornet manganese metabolism with D. F. Waterhouse, he mentioned that, in *Lucilia*, indications were appearing that barium is accumulated much as is copper.[3] This observation, which he has not yet confirmed,[4] prompted selection of barium as the element next to be studied for comparison with manganese.

Since histochemical and microchemical methods for barium are far from satisfactory, this study has been carried on using the radioisotope, barium^{140}, obtained from Oak Ridge, as indicator of the normal pathways of barium through the body. It is not felt that any defense of this procedure is necessary, except to note that the available analyses of plant material for this element[5] indicate its general availability to any organism which can absorb it, and that Ballard[6] has provided spectrographic evidence of barium as a normal constituent of two insects.

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The distribution of ingested barium^{140} within the organisms was studied both by conventional Geiger-counter examination of dissected organs, and by radioautography. The latter technique employed the method previously described by Evans,[7] and independently developed by several investigators, of mounting histological sections directly on photographic emulsion, which is developed, after suitable time for exposure, with the sections still in place and then mounted under a cover-slip for microscopic examination. It was found that mounting such preparations in a medium of suitable refractive index, glycerine of about 75 per cent concentration having been employed, permitted identification of all tissues and of most cellular constituents, while producing as little interference as possible with the visualization of the underlying photographic grains. Such preparations are extremely revealing and provided the main evidence for the description of the cycle of barium metabolism in hornets. By dividing the series of sections and employing widely differing exposure times, very clear cut pictures could be obtained of localizations differing by 10^4 or more in absolute barium^{140} content.

The barium distribution in the organs of the abdomen of *Vespula* males, at intervals after ingestion of the radioisotope as indicated by an extensive series of radioautographs, is set out in TABLE 1.

TABLE 1

Specimen	1	2	3	4	5	6
Days after feeding	1.8	2.2	3.2	7	14	28
Activity in:						
Mid-gut epithelium	very slight	high	high	high	high	high
Genital tract	definite	definite	definite	nil	nil	nil
Fat Body	nil	strong	strong	slight	nil	nil
Malpighian tubules near gut	very slight	strong	strong	strong	definite	nil
Malpighian tubules near genital tract	nil	definite	definite	definite	nil	nil
Nerve Cord	nil	nil	nil	nil	nil	nil
Hind-gut	very high	high	high	nil	nil	nil

Some more precise localizations are notable. The activity in the fat body is evident only where this tissue lies closely apposed to the mid-gut wall. This is true also of the most intense pick-up by the malpighian tubules, which, in fact, are seen to pick up no barium except when lying adjacent to genital tract or mid-gut. In all of the autographs showing high-level radioactivity in the hind-gut, this is seen to lie within the peritrophic membrane-enclosed fecal pellets, indicating direct passage from the mid-gut, rather than absorption, followed by excretion. The evidences of this second process are clear-cut in later specimens, but involve greatly lower amounts of barium.

It is possible to discern that the malpighian tubules contain barium in both cells and lumen of the upper portions, but that, distally, only cellular barium is present. The possibility that in the lower portion of the lumen barium is more easily lost during histologic preparation must of course, be kept in mind.

The very high concentrations of barium in the mid-gut epithelium are seen to be undergoing slow redistributions. In the earliest stages, the activity is restricted to the distal border of the cells and shows great variation in intensity, some groups of cells showing many times as much activity as those lying immediately adjacent. In the later specimens, the activity is seen to spread slowly toward the proximal end of the cells, and the intensity variation to be minimized. Even by 28 days, however, the proximal one-fifth of the cells has not been entered by the stored barium. This discrimination is made possible by the fact that the edge of the localization can be determined to within about 4 micra, in suitable preparations.

On certain specific points, this evidence is supplemented by the counting results. It was shown clearly that the loss of barium by passage to the hind-gut proceeds much more rapidly than does the absorption of barium by the mid-gut epithelium. From 1.5 to 2 days, when virtually no barium is yet in the epithelial cells, the hind-gut already contains as much of the element as is present in both cells and lumen of the mid-gut.

TABLE 2

Very High	10^9 atoms
High	10^8 atoms
Strong	10^7 atoms
Definite	10^6 atoms
Slight	10^5 atoms
Very Slight	probably less than 10^4 atoms

By use of radium D and E, samples prepared and calibrated by the National Bureau of Standards, estimates were made of the total number of atoms of barium^{140} present in the various organs. In TABLE 2 are set out the number of atoms present in the entire organ, studied as a dissection, corresponding to the descriptive terms employed in TABLE 1. This is only an order of magnitude correspondence, due to differences in volume among organs, but is of interest as indicating the extreme sensitivity of the method, as well as the extraordinary delicacy with which the hornet distributes its barium.

In this study, advantage was taken of the fact that barium^{140} decays into lanthanum^{140} which is in turn radioactive and decays into stable cerium^{140}. Since the half-lives of the two processes are respectively 300 hours and 40 hours, any aged sample of barium^{140} is in equilibrium with a definite quantity of lanthanum^{140}, and the decay rate of this equilibrium mixture is that of the first transition. If now this mixture is subjected to a process removing a part of the lanthanum, the decay curve of the remainder will be altered so that for a period of time its activity will increase. On the other hand, if this mixture is subjected to a process removing a part of the barium, the decay curve will be altered so that for a period of time its activity will decrease more rapidly than expected.

Such decay curves made on various dissections of specimens in this series indicate that the radioactivity in the genital tracts of 1.5 day specimens was pure barium^{140}, whereas a specimen taken 4 days after feeding was excreting a mixture somewhat enriched in lanthanum^{140}.

The cyles of barium distribution in male hornets, inferred from the studies briefly summarized above, are diagrammed in FIGURE 1. Detailed exposition of the arguments is not possible here. In the case of the most interesting feature: The existence of two cycles, chemically distinct and operating at different times, the reasoning was based on the following points. Barium appeared in separated form in the genital tract before radioautographs revealed it in any other organ. Although it must have been transported to this point in the hemolymph, no uptake by the malpighian tubules was shown until after this early barium had passed through the genital tract. On the other hand, the later barium, mostly retained by the mid-gut epithelium, when transmitted to the hemolymph, was immediately absorbed by the adjacent malpighian tubules, and never reached the genital tract. On the diagram, these two points are indicated by the dashed steps, 15 and 16, which do not occur.

Another feature of great interest is the similarity between the treatment of barium by the mid-gut epithelium as inferred here, and the ferritin cycle described for iron in mammalian intestine by Granick.[8] No evidence of control of further barium absorption, by building up of the stored form, has yet been seen, but in other details the similarity is striking. Other intestinal accumulations like that of iron as ferritin have been described for copper in blowflies by Waterhouse[3] and for manganese in hornets by Bowen.[9]

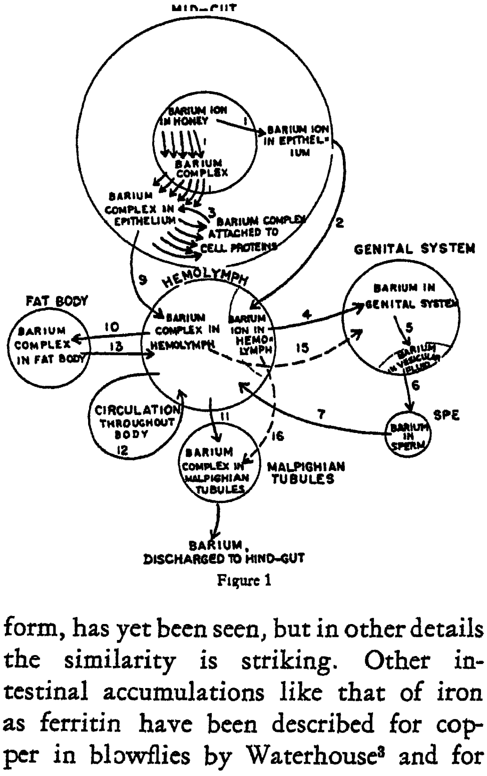
MID-GUT
BARIUM ION IN HONEY
1
BARIUM ION IN EPITHELIUM
1
BARIUM COMPLEX
BARIUM COMPLEX IN EPITHELIUM
3
BARIUM COMPLEX ATTACHED TO CELL PROTEINS
2
GENITAL SYSTEM
9
HEMOLYMPH
FAT BODY
BARIUM COMPLEX IN FAT BODY
10
13
BARIUM COMPLEX IN HEMOLYMPH
BARIUM ION IN HEMOLYMPH
4
15
BARIUM IN GENITAL SYSTEM
5
BARIUM IN VESICULAR FLUID
6
CIRCULATION THROUGHOUT BODY
12
11
7
16
SPE
BARIUM IN SPERM
BARIUM COMPLEX IN MALPIGHIAN TUBULES
MALPIGHIAN TUBULES
BARIUM, DISCHARGED TO HIND-GUT

Figure 1

Thus far, no evidence for physiological function of barium has been presented, and none is available from the present study. It is attractive, however, to postulate that the low-level cycle involving the male genital system, may indicate barium as being a physiological element. It is hoped that this problem may be attacked experimentally during the 1949 hornet season.

The studies reported so far involved only males. During the past fall, at Brookhaven National Laboratory, a similar series of experiments has been carried out, on an expanded scale, on both males and queens of the genera *Vespa* and *Vespula*. Although most of the radioautographs have yet to be completed, very definite differences are evident between the two sexes, not merely in relative distribution of barium, but in rate and amount of uptake. This is the more interesting, as no such differences with respect to manganese were noted in the work cited above[9].

The two series of experiments will provide a very solid foundation on which to build a picture of the chemical states and possible function of barium in hornets and perhaps in insects in general.

The major amount of the work described here was performed at Yale University during the author's stay as Research Assistant in Biogeochemistry, supported by the Survey of Contemporary Knowledge in Biogeochemistry of the American Museum of Natural History. A part of this work was presented in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Zoology at Yale University. As indicated above, the experiments are being continued at the Biology Department of Brookhaven National Laboratory.

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Note. There were no meetings in December of the Division of Mycology, the Section of Psychology, and the Section of Anthropology because of the conflict with the holiday season.

SECTION OF PHYSICS AND CHEMISTRY

INFORMATION OBTAINED FROM THE INTERACTION OF SLOW NEUTRONS WITH MATTER

By JAMES RAINWATER*

In recent years, the physicist has discovered in the slow neutron an excellent tool for the study of the atomic and nuclear properties of matter. It is the purpose of this article to review briefly some of the information which can be obtained from such studies.

Slow neutrons are not produced directly, but are obtained by slowing down the fast neutrons produced by nuclear reactions in, for example, a cyclotron or a chain reacting pile. The neutrons, when produced, have rather high kinetic energies, corresponding to $\sim$ 0.1 to 10 MEV (1 million electron volts = 1.603×10^{-6} ergs). They may be slowed down by elastic "billiard ball" type collisions with light nuclei, such as the hydrogen in paraffin or water. When a beam of fast neutrons impinges on a slab of paraffin, some pass through without effect but some have one or more collisions in the slab with a decrease in kinetic energy at each collision. The energy thus "cascades" to lower values until the kinetic energy of the neutron is of the same order of magnitude as the thermal energies of atoms in the paraffin ($\sim$ 0.04 e.v.). When a neutron has slowed to thermal energies it is apt to gain as easily as lose energy in a collision and comes into thermal equilibrium with the atoms. These "thermal neutrons" then diffuse through the slab until they escape from the surface or are captured by some atom in the slab to produce a nuclear reaction. In paraffin, this capture is mainly by hydrogen and produces a deuteron (hydrogen of mass 2) with the release of 2.23 MEV energy (the binding energy of the deuteron).

The net result of the above processes is that a flux of neutrons leaves the slab, consisting of neutrons of all energies from zero to the initial energy of the fast neutrons. The shape of this spectrum is conveniently broken into a "cascading" region above about 0.3 e.v. and a "thermal" region which corresponds to a Maxwell-Boltzman distribution for the temperature of the paraffin and which centers about $\sim$ 0.04 e.v. for room temperature. The paraffin slab, under such conditions, may be considered as an effective "source" of slow neutrons. Table 1 lists the relations of interest. For neutron energies given in the first row, the second row gives the neutron velocity in meters/sec., and the next gives the inverse (which is important for velocity selector measurements) expressed in microseconds per meter. In a slow neutron velocity selector system, neutrons of different energies are selected by the difference in time required for them to travel a fixed "source-detector distance" of $\sim$ 5 or 6 meters. The fourth row shows the time in microseconds for a neutron to travel 5.4 meters. For thermal energies it is seen that the timings are conveniently large (in microsecond units).

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The next row gives the wavelength of the neutron. According to our present views based on wave mechanics, all particles have the properties both of a wave motion and of particles. This is true of all particles, such as neutrons, protons, electrons, and photons (electromagnetic energy). The wave length is given by DeBroglie's relation $\lambda = h/p = h/mv$. Here h is Planck's constant and p is the momentum of a particle of mass m and velocity v. The important thing to notice is that λ varies as $1/v$ and is of the same order of magnitude as the spacing of atoms in solids for neutrons of thermal energies. This means that one can expect diffraction and interference effects using slow neutrons just as for X-rays. This will be discussed in greater detail later.

FUNDAMENTAL RELATIONS IMPORTANT IN THE MEASUREMENTS DESCRIBED IN THIS PAPER

E→	1 Mev	0.1 Mev	0.01 Mev	1000 ev	100 ev	10 ev	1 ev	0.1 ev	0.01 ev	0.001 ev
V in meters/sec.	1.38×10^7	4.38×10^6	1.38×10^6	437,500	138,400	43,750	13,840	4375	1384	438
Microsec./meter	.0723	.229	.723	2.29	7.23	22.9	723	229	723	2286
Microsec./5.4 meters	.390	1.24	3.90	12.4	39.0	124	390	1235	3900	12,350
λ in A	.0003	.001	.003	.0091	.0286	.0905	.286	.905	2.86	9.05
Collisions required to cascade from 7 Mev	2	4	7	9	11	14	16	18	21	23
Assumed cm m.f.p.	4	1.3	.8	.7	.7	.7	.7	.4	.2	.2
Microsec. time required to cascade	.005	.01	.03	.06	.2	.7	2	4	9	20

The sixth row gives the mean number of collisions with H atoms required for a neutron to cascade to the given energy starting at 7 MEV. The next row gives the mean free path between collisions at the given energy, and the last row gives the time required to cascade to the given energy. By comparing this time with the time to travel 5.4 meters, it is seen that the cascading time is relatively small and can be neglected.

Most scientists are very familiar with the type of information obtained from the scattering of X-rays. Each element of volume in a sample scatters with an amplitude proportional to the density of electrons at the point and with a phase which depends on the position of the volume element and on the X-ray wave length and the direction of scattering. For a crystalline material, the directions in which strong scattering occur are those satisfying the Bragg condition $n\lambda = 2\ d \sin\ \theta$ for the crystal planes. In particular, the scattered amplitude, as a function of scattering direction, is just the Fourier transform of the electron density *vs.* position in the material. Thus, by taking the Fourier transform of the observed scattering amplitude function, one obtains, directly, information concerning the electron density vs. position for the material. This gives most important direct evidence for the size and shape of the electron distribution in atoms. It also shows the distribution of atoms in crystals to give direct information concerning crystal structure and atomic spacings. Recently, these techniques have been applied to the study of the shapes of complex organic molecules. A British group, for example, has now established the atom "map" for penicillium and is attempting to unravel the structure of the red blood cells.

Whereas the scattering of X-rays is by

the electrons in an atom, the scattering of slow neutrons is by the nuclei. Also, where the total scattering of X-rays depends only on the number of electrons in an atom and increases in proportion to the atomic charge number, the magnitude of the nuclear scattering varies in a more or less random manner from element to element depending on specific nuclear interaction effects. Thus, if the magnitude of the interaction is expressed as a "cross section" (cm^2 per atom), the cross section for H is $\sim$ 20 barns (1 barn = 10^{-24} cm^2/atom) while Bi has only 9 barns. Thus, the scattering by neutrons "weights" different atoms quite differently than does the X-ray scattering. For example, it is very hard to locate H atoms by X-rays since they have only one electron for scattering. The extra large scattering cross section of H for neutrons would make it seem that the position of H atoms could be determined easily using neutron scattering, but it turns out that the scattering by H is mainly "incoherent" and it is not as useful as would be expected. However, if the ordinary hydrogen atoms are replaced by deuterium atoms, this trouble is removed and H positions can be determined.

The investigation of slow neutron diffraction has been carried out mainly in conjunction with the chain reacting piles, since an intense flux of slow neutrons is required. Crystal spectrometers for slow neutrons, similar to those for X-rays, have been developed. In general, we may say that the resolution and intensity obtainable with slow neutrons is much poorer than with X-rays, since an X-ray tube is a much more intense source of X-rays than is a pile of slow neutrons. A considerable number of neutron diffraction studies have been started recently and the results of the new technique should become increasingly evident in a few years as the techniques are developed and exploited.

The neutron has no electric charge, but, like the electron, it has a "spin" of $\frac{1}{2}$ unit and an associated magnetic dipole moment. Due to its magnetic moment, there is an interaction with the electron magnetic moment of paramagnetic and ferromagnetic atoms. A beam of slow neutrons passing through a magnetized iron plate is attenuated to a different extent if the neutron spin points in the direction of the magnetic field than if it points in the opposite direction. Thus, a partially polarized beam of slow neutrons may be produced by passing through magnetized iron. The degree of polarization produced depends in a sensitive way on the magnetic domain size and the nearness to magnetic saturation. Such studies have recently given valuable information concerning certain problems of ferromagnetism. The production of polarized slow neutrons has also been used as an analytic method for the precision measurement of the magnetic moment of the neutron which is of great interest to nuclear physics. In a similar manner, the investigation of the transmission of slow neutrons through paramagnetic materials such as MnF_2 promises to give information concerning the effective size of the electron shells responsible for paramagnetism and to show the extent of the local couplings between the different paramagnetic ions.

Neutron diffraction studies also provide information of interest to nuclear physics. The amplitude and *sign* of the scattering of nuclei can be determined from the relative intensity of the Bragg scattering from different crystal planes. This can, in general, be different for different isotopes of a given element and can even be different for a given nucleus depending on how the neutron and nuclear spins combine in the collision. If the scattering amplitude is different for different isotopes, or due to different spin orientation, the intensity of the "coherent" scattering from crystal planes is reduced and there is an increased incoherent scattering. This is particularly true for hydrogen, where the sign of the scattering is opposite for the two spin orientations, so there is a very large incoherent scattering but very little coherent scattering.

The experiments with which the author has been concerned have involved the use of the Columbia University cyclotron modulated to act as a velocity selector system. The operation may be explained in terms of a specific example. The cyclotron arc (ion source), normally turned off, is turned on for a 4-microsecond interval and then turned off again. This produces a burst of deuterons at the center of the cyclotron which are accelerated and strike the beryllium target a few microseconds later to give a burst of fast neutrons leaving the Be in all directions. A certain fraction of the thermal neutron spectrum reaches a BF_3 proportional counter slow neutron detector situated at the end of a collimated path ending outside the water tank barrier surrounding the cyclotron. Samples to be studied can be placed in the path between the source and detector. The slow neutron transmission (or cross section) of the material is measured by observing the factor by which the counting rate is decreased when the sample is in place. The pulses at the counter will occur whenever a neutron is detected and the time difference between the arc and detection pulses, after suitable corrections, gives the "time of flight" for the neutron to travel the known source detector distance, and thus, from TABLE 1, the neutron energy. By using a system of "gating" circuits, the counts coming in different timing intervals (4 microseconds wide in this example) may be recorded on different registers and, thus, the different energy groups can be separated. The Columbia velocity selector is arranged to measure 16 different energy intervals simultaneously for each of two detector-collimator systems. The geometric arrangement is illustrated in FIGURE 1.

In any spectrometer system the shape of the source spectrum is important. FIGURES 2 and 3 show the intensity *vs.* time of flight of the neutrons from paraffin slabs of two different thicknesses. The relative number of neutrons detected in equal time of flight intervals is shown. An energy scale is given for convenience. (The high energies or high velocities correspond to small timings and vice versa.) The peak in the curves is due to the Maxwell-Boltzman distribution for which the solid line gives the theoretical curves. The rise above the solid curve at smaller timings corresponds to the "cascading" neutrons. It is seen that relatively more of the neutrons reach thermal energies with the thick slab.

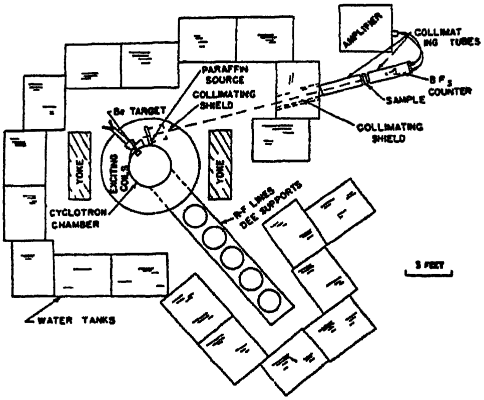

Figure 1 Room plan showing the cyclotron, the water tank shielding the collimating system, and the slow neutron "source," sample, and detector positions.

The relative number of thermal neutrons in the slab at any time after the burst decreases exponentially in time because of losses due to diffusion and capture. This is shown in FIGURE 4 for the same two source slabs. The decay is faster for the thin slab. The observed values agree with the calculated values. The thermal lag must be deducted for measurements below about 0.2 e.v.

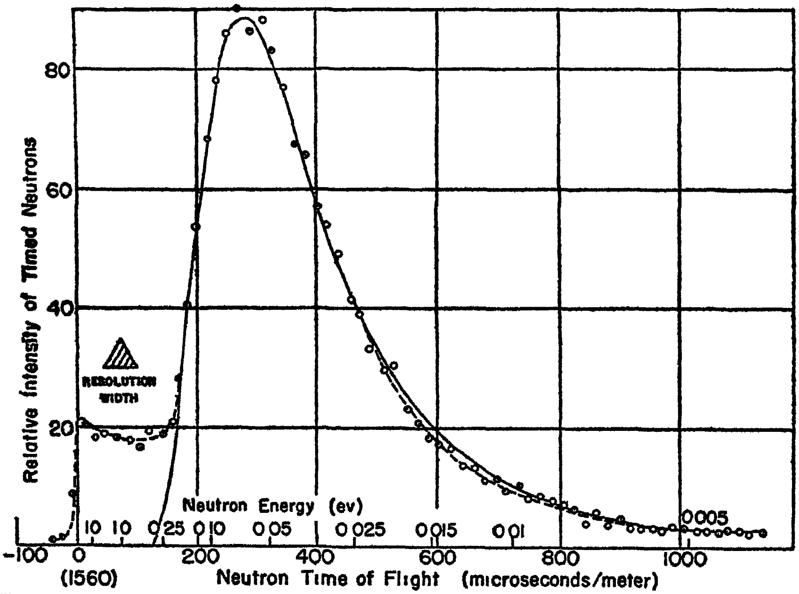

Figure 2 The slow neutron distribution from a thick paraffin source slab (6.2 cm thick paraffin in a box of ½ thick plywood) O = experimental curve — = v^4 theoretical curve for 390°K

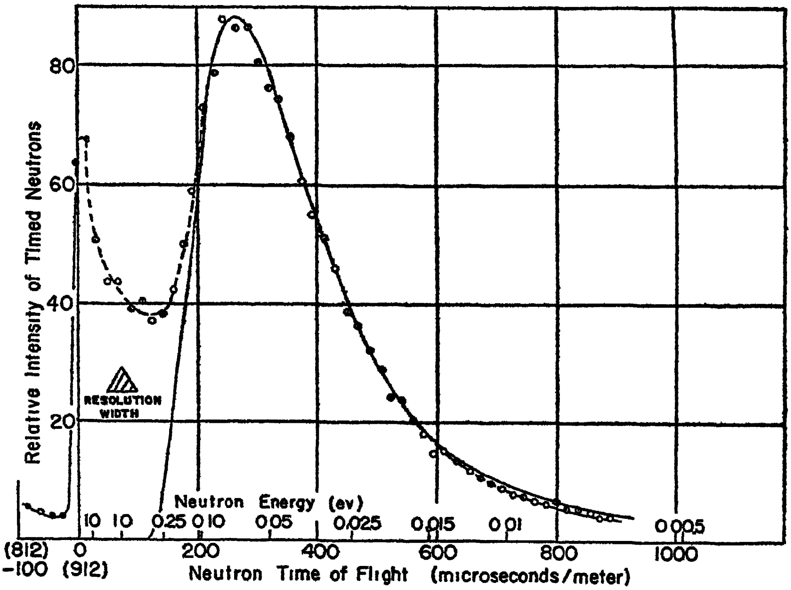

Figure 3 The slow neutron distribution from the thin paraffin source slab (2.6 cm thick paraffin in a box of ½′ thick plywood) O = experimental curve — = v^4 theoretical curve for 430°K

The main use of the velocity selector system, however, has been to investigate the interaction of slow neutrons with nuclei, since the variation of this interaction with energy supplies much of the detailed knowledge of the energy levels of these nuclei. Absorption bands are found at low neutron energies which give a picture of the levels of the compound nucleus in the vicinity of the binding energy of the neutron The method thus permits a microscopic examination of the level structure of the compound nucleus at a region of ∼ 8 MEV excitation and provides some of the main experimental evidence concerning level structure

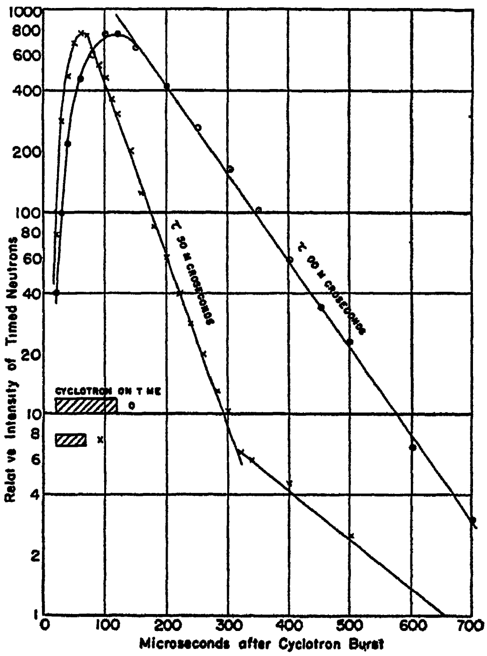

Figure 4 Thermal neutron delayed emission from paraffin source slabs Data taken next to the source with a tiny BF₃ counter X = 2 6 cm thick paraffin in a box of ½″ thick plywood O = 6 2 cm thick paraffin in a box of ½′ thick plywood

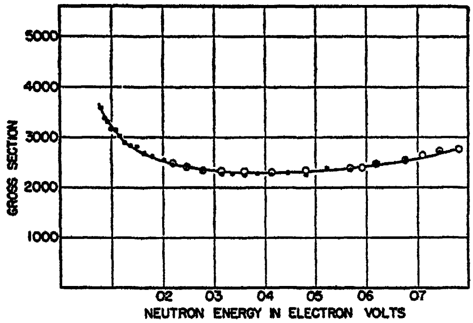

Figure 5 The slow neutron cross section of cadmium in the energy interval 0-0 08 ev ● experimental points for the 43 8 mg/cm² sample O, experimental points for the 219 mg/cm² sample The solid line is a theoretical curve using the Breit Wigner one level formula with E_0 = 0 176 ev, Γ = 0 115 ev, and σ_0 = 7200

The Breit-Wigner formula predicts that the absorption should show a resonance maximum at exact resonance and a falling off away from resonance It also predicts an additional factor proportional to $1/v$ The most exact check of this formula was made in the case of Cadmium and is shown

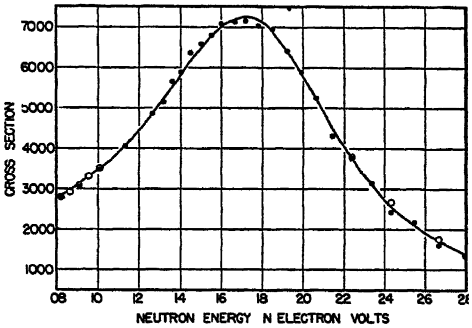

Figure 6 The slow neutron cross section of cadmium in the energy interval from 0 08 ev to 0 28 ev ● experimental points for the 43 mg/cm² sample O experimental points for the 219 mg/cm² sample The solid line is a theoretical curve us ng the Breit Wigner one level formula with E_0 = 0 176 ev Γ = 0 115 ev and σ_0 = 7200

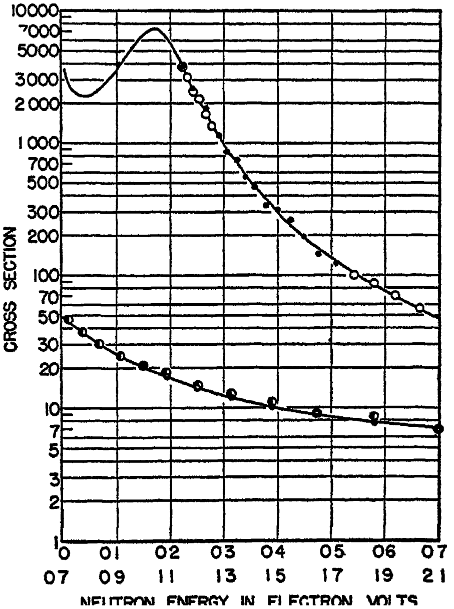

Figure 7 The slow neutron cross section of cadmium in the energy interval from 0 2 ev to 2 1 ev ● experimental points for 43 8 mg/cm² sample O, experimental points for 219 mg/cm² sample ◐ experimental points for 4 47 g/cm² sample ⊖ experimental points for 11 03 g/cm² sample The solid line is a theoretical curve using the Breit Wigner one level formula with E_0 = 0 176 ev Γ = 0 115 ev and σ_0 = 7200 A constant term of 5 3 has been added to the theor t cal curve to account for the scattering cross section

in FIGURES 5, 6 and 7. The agreement of the experimental points and the (single) best fitting theoretical curve is shown over the energy range from 0.008 e.v. to 2.1 e.v. The level is at 0.176 e.v. and has a width of 0.115 e.v. with a cross section of 7200 barns at maximum for normal Cadmium (or ~ 8 times as much for the responsible Cd^{113} isotope). At higher energies the resonances show up as dips in the transmission curve of the sample. FIGURE 8 shows the results for two thicknesses of tantalum showing levels at 4.1, 10, 13, 22, 37, and many near 100 to 300 e.v. neutron energy. FIGURE 9 gives the results for Cobalt showing the $1/v$ slope at low energies and the strong resonance at 115 e.v. This is shown better in FIGURE 10 which also shows a level near 10,000 e.v. Such curves have now been obtained for a large number of elements. The results for Cadmium permit the calculation of the mean life of the compound nucleus for the emission of a γ ray and for the emission of a neutron. The other curves also give information on level density

Nuclear physics differs from atomic physics in that the law of force acting between nucleons to give nuclear binding is not known. From a number of measurements, it is known that the force is strong within ~ 2 × 10^{-13} cm. and decreases to zero very rapidly at larger distances. The neutron-proton interaction, or deuteron problem, represents the fundamental two body interaction in nuclear physics analagous to the H atom in atomic physics. For any given assumption concerning the force law, the two body problem can be reduced to a one body problem and solved

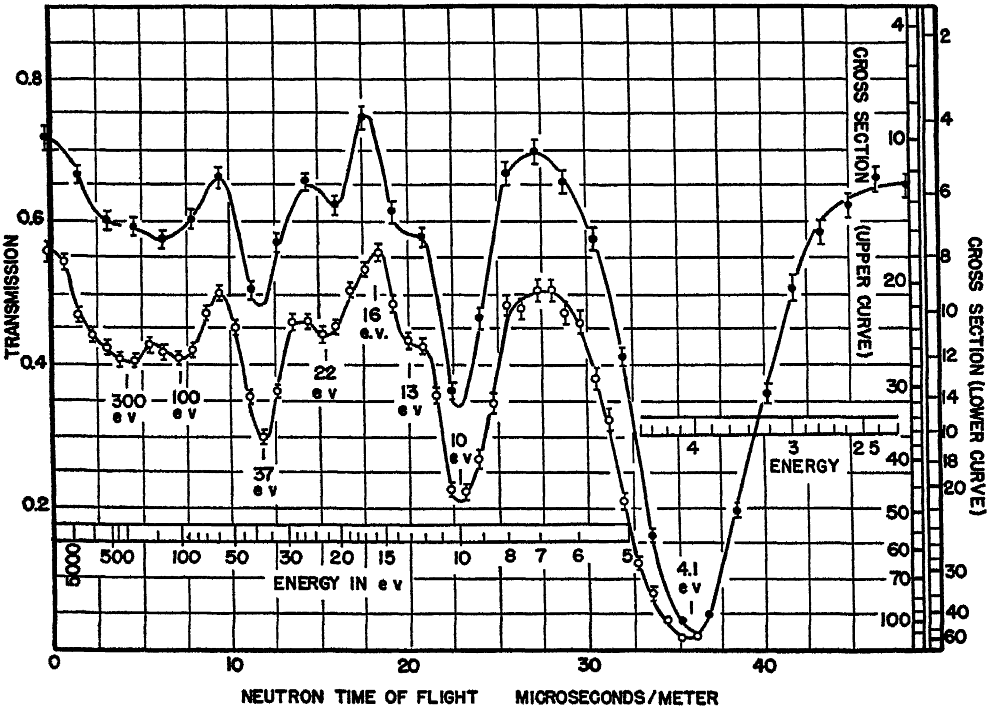

Figure 8 The slow neutron transmission of tantalum. The upper curve shows the transmission for a 9.98 g/cm² sample with a resolution width of 3.6 μsec/meter. The lower curve shows the transmission for a 22.44 g/cm² sample with a resolution width of 1.8 μsec./meter. The use of the thicker sample with higher resolution increases the detail that can be obtained.

exactly. Thus, all information concerning the neutron-proton interaction is of utmost interest to nuclear physics. The neutron and proton are known to combine to form a stable system (deuteron) of 2.23 MEV binding. This is a triplet state corresponding to parallel spins. Assuming any given range for the nuclear forces and knowing the binding energy of the deuteron, it is possible to calculate exactly the (triplet) slow neutron-proton cross section. The predicted result is ~ 4 barns compared to the experimental value of (20.36 ± 0.10) barns. The reason for this difference is that the nuclear forces are spin dependent and the interaction is only about half as large for opposite spins in the singlet interaction. This turns out to not quite give binding for the singlet interaction but to give a near resonance cross section of ~ 70 barns. Weighting the triplet interaction $\frac{3}{4}$ and the singlet interaction $\frac{1}{4}$ gives the observed value. The sign of the scattered amplitude is opposite in the two cases, as is shown by scattering from crystals containing hydrogen and by the ortho-para hydrogen experiment. H_2 molecules can exist as ortho-hydrogen in which the two proton spins are parallel, or as para-hydrogen in which the proton spins are opposed. Interference effects in the scattering of very slow neutrons by H_2 gas at ~ 20°K result in a cross section of ~ 4 barns for para-hydrogen and ~ 125 barns for orthohydrogen. In terms of the binding energy of the deuteron and the "free proton cross section" (20.36), it is possible to calculate the expected para-hydrogen cross section for any chosen value of the range of the nuclear forces and this provides the most precise evaluation of the range of the neutron proton force.

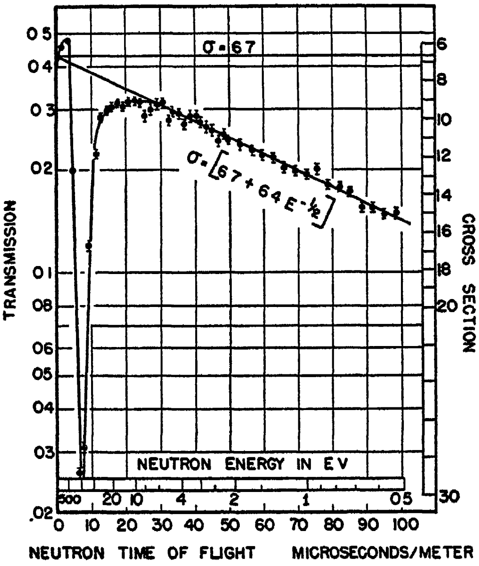

Figure 9 The slow neutron transmission of 12.4 g/cm² of cobalt. This figure shows a strong dip near 100 ev and another small dip at zero time of flight.

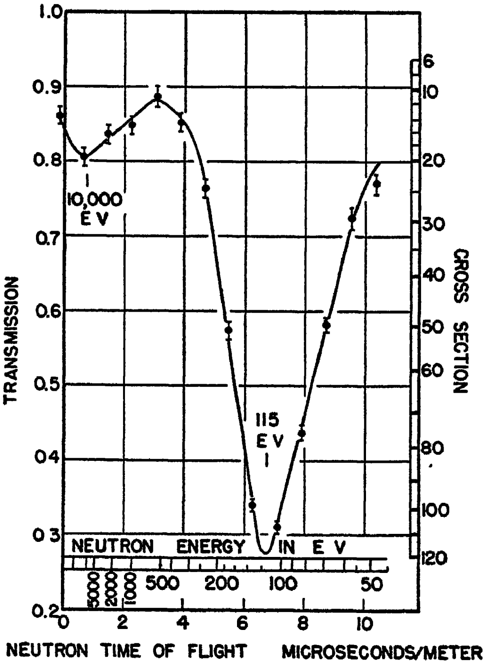

Figure 10. The slow neutron transmission of 1.06 g/cm² of cobalt. This curve was taken by using a thinner sample and maximum resolution.

Recently, we have begun an experiment to measure the neutron-electron (nonmagnetic) interaction by an indirect method involving a very precise measurement of the slow neutron cross-section *vs.* energy of liquid lead and bismuth. This interaction is of great importance since it gives direct evidence concerning the meson theory of nuclear forces. On this picture, a neutron is considered to be a "simple" neutron part of the time and a proton and negative meson part of the time. The electron interacts with the (virtual) proton-negative meson system to give a scattering. The observed magnitude of the interaction is of the predicted order of magnitude. A precise determination of the interaction will help choose between different alternative formulations of the theory. The other data which must be fitted by an adequate meson theory of nuclear forces include the binding energy of the deuteron, the magnetic dipole moments of the neutron, proton, and deuteron, the ortho-para hydrogen scattering proton-proton scattering, and the electric quadrupole moment of the deuteron.

References

1. Rainwater, Havens, Wu, Dunning & Meaker. 1947. Phys. Rev. **71**: 65, 165, 174. 1948. Phys. Rev. **73**: 733, 963.
2. Wollan & Shull. 1948. Nucleonics.
3. Goldsmith, Ibser & Feld. 1947. Rev. Mod. Phys. **19**: 259.

CONFERENCE HELD

SECTION OF BIOLOGY

CONFERENCE ON "THE GROUND SUBSTANCE OF THE MESENCHYME AND HYALURONIDASE": DECEMBER 3 AND 4, 1948

The section of Biology held a Conference on "The Ground Substance of the Mesenchyme and Hyaluronidase." Doctor F. Duran-Reynals, Yale School of Medicine, New Haven, Connecticut, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, December 3

Morning Session. Fundamental Data on the Ground Substance of the Mesenchyme. Chairman: K. Meyer

"The Mucopolysaccharides of the Interfibrillar Substance of the Mesenchyme" by K. Meyer, College of Physicians and Surgeons, Columbia University, New York N. Y.

"A Study of Certain Connective Tissue Constituents with the Electron Microscope" by J. Gross, Massachusetts Institute of Technology, Cambridge, Mass.

"Preparation of Hyaluronic Acid from Various Animal Sources" by H. E. Alburn and E. C. Williams, Wyeth Institute of Applied Biochemistry, Philadelphia, Pa.

"The Distribution of Acid Mucopolysaccharides in Mammalian Tissues, as Revealed by Histochemical Methods" by H. Bunting, Yale University School of Medicine, New Haven, Conn.

"Histological Studies of the Reactions of Cells and Intercellular Substances of Loose Connective Tissue to the Spreading Factor of Testicular Extract" by S. H. Bensley, University of Toronto School of Medicine. Toronto, Canada.

"Solubility Properties of Some Components of the Ground Substance and the Relation to Intravital Staining of Connective Tissue" by H. R. Catchpole, University of Illinois College of Medicine, Chicago, Ill.

"Physiological Conditions Existing in

Connective Tissue" by P. McMaster, Rockefeller Institute for Medical Research, New York, N. Y.

"The Cutaneous Mucopolysaccharides in Localized (Pretibial) Myxedema" by E M. Watson and R. H. Pearce, Faculty of Medicine, University of Western Ontario, London, Canada.

"Some Studies on the Cutaneous Mucopolysaccharide of the Sex Skin of *Macaca mulatta*" by G. Van Wagenen, H. Bunting, and F. Duran-Reynals, Yale University School of Medicine, New Haven, Conn.

Afternoon Session. The Permeability of the Ground Substance in Infection and Other Conditions.

Chairman: D. H. Sprunt.

"The *in vitro* Action of Hyaluronidase" by A. Dorfman, University of Chicago School of Medicine, Chicago, Ill.

"The Action of Hyaluronidase on Hyaluronic Acid" by K. Meyer, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"*In vitro* Studies with Hyaluronidase" by Sibylle Tolksdorf, James W. Cassidy, Marion H. McCready, and D. Roy McCullagh, Schering Corporation, Bloomfield, New Jersey.

"Spreading Factors and Their Mechanism of Action" by O. M. Hechter; Worcester Foundation for Experimental Biology, Shrewsbury, Mass.

"The Action of Hyaluronidase on Extracellular Structures" by R. Chambers and B. W. Zweifach, New York University and Cornell University Medical College, New York, N. Y.

"The Ground Substance in Infection" by D. H. Sprunt, University of Tennessee, Memphis, Tenn.

"The Function of Hyaluronidase in Hemolytic Infection" by B. Sallman and J. M. Birkeland, Ohio State University, Columbus, Ohio.

"The Production of Hyaluronic Acid and Hyaluronidase by Some Strains of Group A Streptococci" by R. M. Pike, Southwestern Medical School, Dallas, Texas. (Read by title)

Saturday, December 4

Morning Session. The Permeability of the Ground Substance in Infection and Other Conditions (Concluded).

Chairman: M. Lurie

"Mechanisms Affecting Spread in Tuberculosis" by M. Lurie, Henry Phipps Institute, Philadelphia, Pa.

"The Suppression of Experimental Rickettsial Infection by Anti-organ Sera in the Presence of Testicular Extract" by L. Anigstein and D. M. Whitney, University of Texas, Medical Branch, Galveston, Texas. (Read by title)

"Inhibition Effect of Blood Serum on Hyaluronidase" by A. Dorfman, University of Chicago School of Medicine, Chicago, Ill.

"Inhibition of Hyaluronidase by Serum" by Z. Hadidian, Tufts Medical College, Boston, Mass.

"Hyaluronic Acid—Hyaluronidase and the Rheumatic Diseases" by C. Ragan and K. Meyer, College of Physicians and Surgeons, Columbia University and Presbyterian Hospital, New York, N. Y.

"Inhibition of Streptococcal Hyaluronidase by Human Sera" by J. G. Friou and R. W. Quinn, Yale University School of Medicine, New Haven, Conn.

"Mucolytic Enzymes and Invasion by Carcinomas" by W. L. Simpson, Detroit Institute of Cancer Research, Detroit, Mich.

"Use of the Streptococcal Decapsulation Test as a Measure of Thermolabile Hyaluronidase Inhibitor in Serum" by J. K. Fulton, S. Marcus, and W. D. Robinson, University Hospital, University of Michigan, Ann Arbor, Mich.

Afternoon Session. Pharmacology and Therapeutic Applications of Hyaluronidase. Chairman: J. Seifter.

"Studies on Pharmacology and Toxicology of Hyaluronidase" by J. Seifter and J. J. Christian, Wyeth Institute of Applied Biochemistry, Philadelphia, Pa.

"The Influence of Hyaluronidase on the Course of Certain Bacterial and Viral Infections" by G. H. Warren, Wyeth Institute of Applied Biochemistry, Philadelphia, Pa.

"The Use of Hyaluronidase with Local Anesthetic Agents in Surgery and Dentistry" by C. H. Kirby, J. P. Looby and J. E. Ekenhoff, School of Medicine and Dentistry, University of Pennsylvania, Philadelphia, Pa.

"The Clinical Use of Hyaluronidase in Hypodermoclysis and Urography" by C. L. Burket and P. Gyorgy, Altoona, Pa., and University of Pennsylavania, Philadelphia, Pa.

"Further Observations on the Value of Hyaluronidase in the Treatment of Human Infertility" by R. Kurzrock, New York, N. Y.

"A Statistical Analysis of the Interrelationships of Spermatozoa Count, Hyaluronidase, and Fertilization" by B. Sallman and J. M. Birkeland, Ohio State University and the Ohio Agricultural Experiment Station, Columbus, Ohio.

"Evidence Contra-indicating the Value of Hyaluronidase in Therapy of Human Male Infertility" by M. C. Chang and N. T. Werthessen, Worcester Foundation for Experimental Biology, Shrewsbury, Mass.

REPORT OF THE ANNUAL MEETING

December 15, 1948

The 131st Annual Meeting of the Academy, for the election of Officers and Fellows, the presentation of reports, and the transaction of other business was held at the Hotel Astor on the evening of Wednesday, December, 15, 1948.

The first order of business was devoted to the reports of the Officers and Committees of the Academys.

REPORTS OF OFFICERS AND COMMITTEES

Report of the Recording Secretary. This year, particularly, it is a great pleasure to report on the progress made in our activities, the status of our membership, as well as on the plans for the future development of the Academy.

The scope of this progress is noteworthy because of the wide range of the subjects dealt with at the meetings, the high quality of the papers presented and the scientific calibre of the speakers and discussants.

We wish to express our sincere appreciation for the efficiency and generous cooperation of the chairmen of the Sections, the various chairmen and speakers at the eight conferences and the forty-three stated Sectional meetings held during the past year. One hundred and seventy-seven important scientific papers were read and discussed at these meetings.

At the Annual Meeting, excellent addresses were delivered by President Harden F. Taylor, Mr. George A. Sloan and Doctor W. Albert Noyes, Jr., under the general title "New York as the Crossroads in the World of Science."

Two lectures were also given before Special Meetings of the Academy. The first by H. R. H. Prince Peter of Greece on "Tibetan, Toda and Tiya Polyandry;" the second by Doctor Jean Pasteels of Brussels, Belgium on "The Evolution of Nucleic Acid in the Germinal Cycle of *Ascaris megalocephala* as Studied by Cytochemical Methods."

Our special appreciation is extended to Doctors M. L. Tainter, Raymond N. Bieter and Hans Molitor for organizing and conducting the conference on "Newer Synthetic Analgesics" and for obtaining grants-in-aid to make possible the early publication of the proceedings which have appeared under this title.

Thanks are also due to Doctor Perrin H. Long for his successful management of the conference on "Antibiotics Derived from *Bacillus polymyxa*" and to Doctor M. J. Kopac who developed the conference on "The Mechanisms of Cell Division."

The conference on "Methodology and Techniques for the Study of Animal Societies" was sponsored jointly by The New York Academy of Sciences and The New York Zoological Society, in connection with which we wish to particularly thank Doctor J. P. Scott and the Committee for the Study of Animal Societies under Natural Conditions for the splendid arrangement of the program for this meeting.

The Section of Biology also sponsored the conference on "Aureomycin—A New Antibiotic" for which we acknowledge the wholehearted efforts of the late Doctor Y. SubbaRow and extend thanks to Doctor A. R. Dochez, the Chairman of this conference.

The Section of Oceanography and Meteorology sponsored the conference on "Ocean Surface Waves." Doctor Bernhard Haurwitz is most deservedly to be thanked for organizing the conference and for his success in arranging for a grant-in-aid toward the prime publication costs of the proceedings.

The conference on "Human Engineering" was recognized as of outstanding importance. This meeting was held under the auspices of the Section of Psychology and was ably administered by Doctor Lawrence Edwin Abt.

The twenty-fifth Anniversary of the origin of the Debye-Hückel theory was celebrated by the holding of the conference on "Molecular Interaction," with Doctor Peter Debye as Presiding Chairman. Doctor Raymond M. Fuoss deserves exceptional credit and thanks as the Organizing Chairman.

A Division of Mycology has been established under the Section of Biology in response to the wide interest which has arisen, during the past few years, in this field. The organization of the Division has been carried out through the efforts of Doctor Frederick Reiss.

The future development of the Academy centers around the acquisition of a new home where these conferences and evening meetings may be suitably accommodated, with provision for the efficient administration of the Academy's functions.

The Council of the Academy has announced a Building and Sustaining Fund Campaign for a goal of $1,000,000. Half of this sum will be used to purchase a building, attractive and adequate for our purposes. The remainder will be used as a Sustaining Fund to maintain the new home and expand the Academy's activities.

These plans are clearly presented in the brochure entitled "The New York Academy of Sciences—Past, Present and Future" which has been circulated to the entire membership.

The membership has been assigned a quota of $250,000. One thousand transfers from the Annual Classes to Life Membership, at the established fee of $200 each, will produce the sum of $200,000. Payments of this fee may be made over a period of the next two years, if desired.

It is obvious that to accomplish our purpose the unified support and hearty cooperation of all our Members are indispensable. The Recording Secretary earnestly urges that each individual Member will consider it his responsibility to aid in every possible way to achieve its goal.

During the year 1948, there have been added to our rolls, two Life Members, 29 Sustaining Members, 668 Annual Active Members and 40 Student Members, of which 718 are now in good standing and 21 await qualification through payment of dues. Thus, a total of 739 new Members were added during the year. Five Annual Members were transferred to Life Membership by payment of the Life Membership fee. 352 transfers and 18 reinstatements were also effected. Of the 352 transfers, 336 Associate Members transferred to the Active Membership Class.

The Academy has lost by death 4 Honorary Life Members, 2 Sustaining Members and 13 Annual Active Members.

474 resignations were accepted, 240 names were dropped for non-payment of dues and 3 for not having qualified for membership. This is but a moderate loss considering the increase in dues at the beginning of this calendar year.

The record now stands with a net gain of 21 Members. There are, at present, upon the rolls of the Academy 4030 Members, of whom 547 are Fellows, classified as follows: 1 Benefactor, 1 Patron, 45 Honorary Life Members, 105 Active Life Members, 150 Annual Sustaining Members, 3,637 Annual Active Members, 91 Annual Student Members.

Report of the Corresponding Secretary. Record is made with regret that, due to the death of four Honorary Life Members, the number upon the rolls of the Academy was reduced from 49 to 45 during the past year.

Report of the Editor. Since the date of the last Annual Meeting, December 17, 1947, The New York Academy of Sciences issued 11 ANNALS monographs, comprising a total of 1416 pages. Included in these are 102 papers by 169 authors. The TRANSACTIONS (308 pages) bring the total pagination up to 1724.

The list of the publications issued is, as follows:

ANNALS

Volume 49, Article 2, "*Chromatography*" (14 papers), by H. G. Cassidy, N. Applezweig, S. Claesson, V. R. Deitz, B. J. Mair, A. J. P. Martin, S. Moore, R. L. Peck, W. A. Schroeder, L. Shedlovsky, W. H. Stein, H. C. Thomas, and L. Zechmeister. Pages 141–326. Published February 10, 1948.

Volume 49, Article 3, "*Bioluminescence*" (5 papers), by E. N. Harvey, R. S. Anderson, J. B. Buck, A. M. Chase, H. Eyring, and F. H. Johnson. Pages 327–482. Published April 15, 1948.

Volume 49, Article 4, "*Hemorrhage*" (16 papers) by G. Shwartzman, C. H. Best, R. Chambers, C. S. Davidson, J. H. Ferguson, R. F. Furchgott, I. E. Gerber, M. I. Gregersen, P. György, R. L. Haden, C. Hyman, L. B. Jaques, P. Klemperer, R. E. Lee, C. N. H. Long, D. W. Richards Jr., R. H. Schneider, E. Shorr, H. J. Tagnon, S. A. Thayer, L. C. Underwood, D. C. Van Slyke, A. E. Wilhelmi, and B. W. Zweifach. Pages 483–660. Published May 11, 1948.

Volume 49, Article 5, "*Recent Studies in the Mechanisms of Embryonic Development*" (9 papers) by E. J. Boell, D. P. Costello, S. R. Detwiler, G. Fankhauser, J. Holtfreter, J. S. Nicholas, S. M. Rose, D. Rudnick, and L. S. Stone. Pages 661–866. Published September 24, 1948.

Volume 49, Article 6, "*Current Trends in Clinical Psy-*

chology" (6 papers), by A. W. Combs, H. E. Durkin, M. L. Hutt, J. G. Miller, J. L. Moreno, and F. C. Thorne. Pages 867–928. Published October 8, 1948.

Volume 50, Article 1, "*A Biochemical Hypothesis of the Genesis of Cancer*" (1 paper), by L. A. Pinck. Pages 1–18. Published February 6, 1948.

Volume 50, Article 2, "*The Chemotherapy of Filariasis*" (15 papers), by L. L. Ashburn, F. C. Bartter, R. N. Bieter, F. J. Brady, L. M. Brancone, T. Brey, L. G. S. Brooker, H. W. Brown, E. Bueding, T. A. Burch, M. C. Clark, L. T. Coggeshall, D. B. Cowie, A. C. Cuckler, J. T. Culbertson, R. W. Cunningham, E. M. Cranston, J. J. Denton, S. Halliday, B. K. Harned, R. I. Hewitt, S. Kushner, J. T. Litchfield, Jr., W. L. McEwen, T. H. Maren, J. Oliver-Gonzalez, G. F. Otto, L. Peters, H. M. Rose, D. Santiago-Stevenson, H. W. Stewart, Y. SubbaRow, R. J. Turner, R. E. Vessey, W. S. Wallace, D. E. White, H. N. Wright, and N. N. Yuda. Pages 19–170. Published May 25, 1948.

Volume 50, Article 3, "*The Inhibition of Malarial Relapses by Toxoid of Clostridium Tetani*" (1 paper), by E. Y. Garcia. Pages 171–186. Published June 7, 1948.

Volume 50, Article 4, "*Teleological Mechanisms*" (5 papers), by L. K. Frank, G. E. Hutchinson, W. K. Livingston, W. S. McCulloch, and N. Wiener. Pages 187–278. Published October 13, 1948.

Volume 51, Article 1, "*Newer Synthetic Analgesics*" (16 papers), by M. L. Tainter, H. J. Anslinger, R. C. Batterman, R. N. Bieter, S. Bogolub, K. K. Chen, N. B. Eddy, L. Glassman, B. B. Hershenson, S. A. Hirsh, R. W. Houde, H. Isbell, E. H. Jenney, J. S. LaDue, L. C. Miller, E. E. Nelson, C. C. Pfeiffer, L. H. Rasmussen, E. A. Rovenstine, M. H. Seevers, L. F. Small, R. R. Sonnenschein, and F. F. Yonkman. Pages 1–174. Published November 1, 1948.

Volume 51, Article 2, "*Aureomycin—A New Antibiotic*" (16 papers), by B. M. Duggar, L. Anigstein, J. Beninson, E. A. Bliss, A. E. Braley, R. L. Brickhouse, M. L. Bryer, C. A. Chandler, M. C. Clark, H. S. Collins, R. Cosgrove, H. R. Cox, R. W. Cunningham, A. C. Dornbush, H. F. Dowling, M. Finland, B. K. Harned, C. H. Hine, L. M. Hill, E. H. Lennette, M. H. Lepper, P. A. Little, M. A. Logan, P. H. Long, W. J. McCauley, G. Meikeljohn, T. F. Paine, Jr., E. J. Pelcak, C. W. Price, A. Prigot, W. A. Randall, M. Sanders, E. B. Schoenbach, E. Stokey, Y. SubbaRow, L. K. Sweet, H. M. Thelen, R. E. Vessey, H. Welch, D. M. Whitney, S. C. Wong, L. T. Wright, and N. N. Yuda. Pages 175–342. Published November 30, 1948.

In addition to the articles published during the year, the following are now in press and will be published during the next few months:

Volume 50, Article 5, "*Thyroid Function as Disclosed by Newer Methods of Study*" (12 papers), by J. H. Means *et al.*

Volume 50, Article 6, "*The Adrenal Cortex*" (11 papers), by R. Gaunt *et al.*

Volume 51, Article 3, "*Ocean Surface Waves*" (15 papers), by A. J. Abdullah *et al.*

Volume 51, Article 4, "*Molecular Interaction*" (19 papers), by R. M. Fuoss *et al.*

TRANSACTIONS

Series II, Volume 10, Numbers 1–8, consisting of 308 pages, was completed, printed and distributed each month from November, 1947, to June, 1948, inclusive.

SCIENTIFIC SURVEY OF PORTO RICO

Volume 17, Part 1 of the series, "*The Pelecypoda of Porto Rico and the Virgin Islands,*" by Richard A. McLean, is now in press and will be published during the coming year.

Volume 18, Parts 3 and 4, "*Porto Rican Prehistory (I): Introduction; Excavations in the West and North,*" and "*Porto Rican Prehistory (II): Excavations in the Interior, South and East; Chronological Implications,*" by Irving Rouse, is in the Editor's hands and is being prepared for publication.

During the year, the Academy has distributed 86,989 separate publications and 10,978 complete volumes of the various published series, as follows:

Separate Numbers (including authors' reprints)—*Annals*, 48,254; *Memoirs*, 3; *Special Publications*, 6,600; *Scientific Survey of Porto Rico*, 428; *Transactions*, 29,337; *Bulletins*, 2,367.

Volumes—*Special Publications*, 1,827; *Transactions*, 4,180; *Bulletins*, 4,971.

The Academy has received, from exchange institutions, 2,113 separate publications, which have been duly turned over to the Library of The American Museum of Natural History, in accordance with the purchase agreement between the two institutions. All publications included in the Annals and Transactions, as soon as they are ready for distribution, are sent to the exchange institutions throughout the world.

The total sales value of publications, distributed gratis to the Members, authors, grantors, reviewers, and exchange institutions is $50,603.70.

Report of the Treasurer. During the fiscal year, ending November 30, 1948, greater financial progress has been made by the Academy than ever before.

The income from all sources totaled $92,564.55, of which $3,245.97 has been credited to capital funds, leaving a remainder of $89,318.58, as income for general operating purposes.

The total operating expenses amounted to $98,019.62, which is $8,794.04 in excess of income. However, the unexpended balance of $11,367.60, reported as surplus on November 30, 1947, included $10,000., which covered three grants-in-aid of the publication costs of Special Publications, Volume IV, entitled "The Biology of Melanomas," which appeared early in January, 1948.

It is most gratifying to report that through the member's ready support of the increase in annual dues and the addition of 739 new members, the income from this source amounted to $38,070.00, which is $20,405.23 above that for the previous year.

The income from sales of publications has continued to increase and is $1,967.65 above that received during the fiscal year, 1947.

It should be noted that more than two-thirds of the total of operating costs were devoted to publication costs (i.e., $66,551.70).

The book value of the Academy's investments and cash on hand, as of November 30, 1948, now amounts to $98,921.32, as shown on the balance sheet. There is a net surplus of $2,666.56, above the total of the endowment funds, at the end of the fiscal year. The interest on investments amounted to $4,906.33, representing a yield of 5.2%. Through sales of certain stocks a net capital gain of $2,281.17 was produced.

The ability of the Academy to render the service of which it is capable depends greatly upon a steadily increasing financial status.

Due to careful planning by the Council, with prudent use of the income of the Academy, maximum results have been obtained from the funds available in the production and distribution of publications, increase of membership, and in number and quality of the meetings including an exceptional series of conferences held; but, what has been done is only preliminary to the task yet to be accomplished.

That this has been a year of signal progress is also evidenced by the substantial beginning which has been made on the realization of our long-cherished hopes and plans to secure a permanent home for the Academy with space and facilities suitable for the expansion of its activities until it shall become a center for Science in New York City.

A drive has been developed to obtain one million dollars, $500,000. of which will be devoted to the purchase, equipment, and furnishing of a building. The remaining $500,000., is the amount estimated as necessary to cover the maintenance and upkeep of the building, as well as the expansion of our activities, which would result from improved facilities made possible by the new location.

We are asking our membership to give this enterprise a substantial impetus at the beginning by becoming Life Members, paying the requisite fee of $200., each. If one thousand members respond in this way, $200,000. would be obtained. It is

The New York Academy of Sciences

BALANCE SHEET

November 30, 1948

ASSETS

Cash in Chase National Park		
General Account	$314.48	
Special Funds Account	1,004.18	
Building and Sustaining Fund	1,690.83	$3,009.49
Investments—at cost		72,952.88
Advances for Bldg. Fund Campaign		22,958.95
Total Assets		$98,921.32
Capital Funds		
General Endowments		$83,645.62
Special Endowments		10,918.31
Bldg. & Sustaining Fund		1,690.83
Total Capital Funds		$96,254.76
Surplus		
Unexpended balance 11/31/47	$11,367.60	
Less: Excess of expenses over income availed of for year	8,701.04	
Surplus Balance 11/30/48		$2,666.56
Total Capital & Surplus		$98,921.32

The New York Academy of Sciences

STATEMENT OF INCOME AND EXPENSES

Year Ended November 30, 1948

Income—All Sources		
Membership dues		$38,070.00
Sales of Academy Publications		27,115.29
Contributions & grants for publications and conferences		15,827.16
Sales of dinner tickets		1,546.25
Library exchange, Am. Museum of Nat. Hist.		100.00
Investment income		4,378.71
Net Capital Gains on security sales		2,281.17
Other income, credited to Capital funds		3,245.97
Total Income—All Sources		$92,564.55
Operating Expenses		
Publication costs		66,551.70
General administrative expenses		23,582.69
Equipment purchases		952.43
Meeting and conference expenses		5,580.25
Membership expansion costs		622.99
A. Cressy Morrison prizes		700.00
Preliminary campaign survey—balance		29.56
Total Operating Expenses		$98,019.62
Deduct:		
Total Income—All Sources (as above)	$92,564.55	
Less: Income credited to capital (as above)	3,245.97	
Balance-Income for General Purposes		$89,318.58
Excess of expenses over income availed of in year ending November 30, 1948		$8,701.04

also hoped that some of our members may be willing to contribute larger gifts, ranging from $1000. to $5,000. and more, if possible, that this phase of the goal may be attained more rapidly.

The remaining 3,000 Members, namely, three-fourths of our membership, should contribute sums totaling $50,000., thus bringing the entire contribution of the membership to $250,000. or more, as a nucleus on which we may base our requests for larger contributions from outside sources, in order to obtain the remaining $750,000.

We wish to acknowledge with gratitude and appreciation, on behalf of the Academy, seventeen donations or grants totaling $15,827.16, received during 1948, to aid in defraying both conference and publication expenses, as follows: Lederle Laboratories Division, American Cyanamid Company, $10,419.66; Ciba Pharmaceutical Products, Inc., $1,200; Donner Foundation, $500; Morris and Cuna Fogelman Foundation, $500; Hoffman-LaRoche, $400, Merck & Company, $400; E. R. Squibb & Sons, $400; Sterling Winthrop Inc., $400; The Upjohn Company, $400; Smith, Kline & French Laboratories, $300; Abbott Laboratories, $250; Eli Lilly Laboratories, $200; J. D. Searle & Co., $100; Burroughs-Wellcome Laboratories, $150; Sharpe & Dohme Co., $100; Mr. M. Dole, on behalf of Doctor Grinnell Jones's students, $57.50; J. S. Baker Chemical Co., $50. Our thanks are due also to the generous contributors and grantors who have donated a total of $1,690.83 to the Building and Sustaining Fund and whose timely grants have been made in advance of its official start; and to all others, specifically the membership, who have made our progress possible on a sound financial basis during this past fiscal year.

By order of the Council, the books of account have been checked and certified by a professional firm of auditors.

The financial report of the Academy has been approved by the Finance Committee, as provided in the By-Laws and by the Executive Committee, acting on behalf of the Council.

The property of the Academy was verified by the auditors and the Treasurer's Report examined by these Committees, as of December 13, 1948.

AWARDS OF PRIZES

Two prize competitions were announced for the year 1948: (I) Two prizes of $200.00 each in The A. Cressy Morrison Prize Competition in Natural Science and (II) one prize of $200.00 in The George Frederick Kunz Prize Competition in Geology and Mineralogy.

(I) For the A. Cressy Morrison Prizes in Natural Science of $200.00 each, twelve papers were offered, all of high quality. After careful consideration and consultation with experts in the fields represented, the Committee made the following awards:

(1) A prize of $200.00 to the paper entitled "Effect of Concentration on the Viscosity of Dilute Solutions", by Robert Simha of the National Bureau of Standards, Washington, D. C.*

(2) A prize of $200.00 to the paper entitled "The Relation of the Adrenal Cortex to the Structure and Phagocytic Activity of the Macrophagic System," by Albert S. Gordon and Grace F. Katsh, De-

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* For abstracts of this and other prize papers, see pp. 96–98 of this number of TRANSACTIONS.

partment of Biology, Washington Square College of Arts and Sciences, New York University, New York City.

(3) The paper entitled "Porto Rican Prehistory," by Irving Rouse, Associate Curator, Peabody Museum of Natural History, Yale University, New Haven, Connecticut, was so obviously on a par with the other two papers, that Mr. Morrison has generously come to the aid of the Committee and has donated a third prize of $200.00 as a special award to this paper.

(II) The George Frederick Kunz Prize in Geology and Mineralogy of $200.00 was awarded to the paper entitled "Drainage Evolution in the Appalachians of Pennsylvania," by Henry D. Thompson, Professor of Geology and Head of the Department of Geology and Geography, Hunter College, New York City.

The Committee is pleased to announce that Mr. Morrison has generously renewed his offer of two prizes of $200.00 each, for the two most outstanding papers in a field of natural science covered by the Academy and its Affiliated Societies, to be competed for during the year 1949. Announcement of these awards will be made at the Annual Meeting of the Academy to be held in December, 1949.

ELECTION OF FELLOWS AND HONORARY LIFE MEMBERS

The following Members were elected to Fellowship:

Lawrence Edwin Abt, Ph.D.
A. Elizabeth Adams, Ph.D.
Harry G. Albaum, Ph.D.
A. Ansbacher, D.Sc.
Lawrence Atkin, Ph.D.
James W. Atz, A.B.
Donald Wyckoff Baker, Ph.D.
Robert W. Bates, Ph.D.
Stanley D. Beard, Ph.B.
Alan W. Bernheimer, Ph.D.
Junius B. Bird
Richard J. Block, Ph.D.
Chandler McC. Brooks, Ph.D.
Elmer G. Butler, Ph.D.
Victor Cabasso, D.Sc.
Erwin Chargaff, Ph.D.
Alfred L. Copley, M.D.
Donald P. Costello, Ph.D.
Frederick, Coykendal, C.E., A.M., LL.D.
Lloyd F. Craver, M.D.
Raymond W. Cunningham, Ph.D.
William Dameshek, M.D.
Savino Albert D'Angelo, Ph.D.
Leo M. Davidoff, M.D.
Edwin John DeBeer, Ph.D.
Moses Diamond, D.D.S.
Benjamin Minge Duggar, Ph.D.
Abraham Edelmann, Ph.D.
Sidney Farber, M.D.
Wallace Osgood Fenn, Ph.D.
Maxwell Finland, M.D.
Richard M. Fraps, Ph.D.
Jules Freund, M.D.
Jeanne G. Gilbert, Ph.D.
Anna Goldfeder, Ph.D.
Winifred Goldring, D.Sc.
Kurt Goldstein, M.D.
Samuel M. Gordon, Ph.D.
Ulysses S. Grant, Ph.D.
Paul György, M.D.
Benjamin Harrow, Ph.D.
Alfred C. Hawkins, Ph.D.
Sanford B. Hooker, M.D.
Martin E. Hultquist, Ph.D.
Morris B. Jacobs, Ph.D.
Glenn L. Jepson, Ph.D.
Austin Joyner, M.D.
Erwin L. Jungherr, D.M.V.
Sophia J. Kleegman, M.D.
Vera Koehring, Ph.D.
S. Benedict Levin, Sc.B., E.M.
Michael Levine, Ph.D.
John T. Litchfield, Jr., M.D.
Walsh McDermott, M.D.
John F. Mahoney, M.D.
Floyd S. Markham, Ph.D.

Alfred Marshak, Ph.D.
Edwin B. Matzke, Ph.D.
Rudolph L. Mayer, M.D.
Henry Milch, M.D.
William Montagna, Ph.D.
Stuart Mudd, M.D.
Hans Mueller, Sc.D.
Charles W. Mushett, Ph.D.
Joseph B. Niederl, Ph.D.
Charles R. Noback, Ph.D.
Ernst A. Oppenheimer, M.D.
Sidney Paige
Hans Panofsky, Ph.D.
Elaine P. Ralli, M.D.
Frederick Reiss, M.D.
John E. Scarff, M.D.
Morris Schaeffer, M.D., Ph.D.
Stanley Schneierson, M.D.
Hans Selye, M.D., Ph.D., D.Sc.
Elmer L. Sevringhaus, M.D.
Roman Smoluchowski, Ph.D.
Fred D. Stimpert, Ph.D.
E. L. R. Stokstad, Ph.D.
Edwin Way Teale, A.M.
Selman A. Waksman, Ph.D. Sc.D.
Robert K. Waller, M.D.
J. Franklin Yeager, Ph.D.
Frederick F. Yonkman, Ph.D., M.D.
Alexander Zeissig, D.V.M., Ph.D.
Benjamin William Zweifach, Ph.D.

Honorary Life Membership was conferred upon the following eminent scientists:

Hans Cloos, Professor, Geology and Paleontology, Reinische Friedrich Wilhelms Universitats, Bonn, Germany.

Sir Howard Walter Florey, Professor of Bacteriology, Sir William Dunn, School of Pathology, University of Oxford, England.

Ludwik Hirzfield, Professor and Head of the Department of Microbiology, School of Medicine, University of Wroclaw, Poland.

Constantin Levaditti, Director of Institut Fournier, Paris, France.

Henri Pieron, Director and Professor, Institut de Psychologie, University of Paris, France.

Gaston Leon Ramon, Retired Director, Pasteur Institute at Garches, S. et G., France.

Rudolf Stefan Jan Weigl, Director of Microbiology Institute, University of Cracow, Poland.

ELECTION OF OFFICERS

The following officers were elected for the year 1949:

For President

Victor K. LaMer

For Vice-Presidents

George B. Pegram
Joseph C. Hinsey

For Chairmen of Sections

Maurice Ewing
Charles R. Schroeder
Frederick Reiss
Clairette P. Armstrong
Junius B. Bird
Leo Shedlovsky
Hans Panofsky

For Recording Secretary

Harry A. Charipper

For Corresponding Secretary

Ralph H. Cheney

For Treasurer

Robert F. Light

For Editor

Roy Waldo Miner

For Councilors (1949–1951)

Walter Root
John Tee Van
W. G. Bywater

For Finance Committee

Addison Webb, *Chairman*
Joseph W. Barker
Gordon Billiard

PROGRAM OF THE EVENING

After the Business Meeting, the following program was given:

Presentations. A portrait of Sir Alexander Fleming, discoverer of Penicillin, was presented to the Academy by the artist, Bernard Godwin, acting on behalf of an anonymous donor. After the Academy has secured its proposed new building this portrait will be hung there in a place of honor.

America's Future Oil Supplies. By ROBERT E. WILSON, Chairman of the Board, Standard Oil Company (Indiana)

Recording Sounds of Undersea Life. By MILTON B. DOBRIN, Department of Geology, Columbia University

RECORDING SOUNDS OF UNDERSEA LIFE

By MILTON B. DOBRIN[*]

The fact that a geophysicist has been asked to give an address on a subject most properly classified under biology shows how the established division lines between the branches of science are no longer applicable to many modern lines of research. It is fortunate, for this reason, that there are organizations such as the New York Academy of Sciences, where the emphasis is on the unity of scientific knowledge rather than its artificial partitioning.

The ability of some fish to make noise under water has been recognized since the time of the ancient Phoenicians, whose fishermen are said to have located schools of drumfish in the Mediterranean by listening for their sounds. Primitive fishermen in the Malayan seas still make use of an ancient technique described in a communication from David G. Stead of Australia as follows:

"The head man of the fishing crew gets out over the side of the fishing prau and holds on with his head under water, listening until he locates (by their honking) the fish. The net is then cast round them on the bottom. Meanwhile a very heavy gong is being operated on by a man in the prau, and the booming and thrumming from this are conveyed through the water to the fish, which huddle together and are more easily caught in the bag of the net."

In this country, even the common names of many fish species, such as the croaker, hogfish, toadfish, red drum, and grunt, are indicative of their sonic abilities. Fishermen along our east coast and Gulf coast have heard these and other noise-making fish sound off as they were pulled out of the water in nets. In spite of this, the fact that fish make noise at all does not seem generally known, as the continued popularity of the expression, "silent as a fish," testifies.

Until the start of the recent war, the subject of noise production by marine life was virtually unexplored by science. It required the exigencies of the war to point up how little information was available in the scientific literature on the nature and magnitude of sounds created under water by fish and crustacea. Such information became necessary when it was discovered that biological noises were often interfering with the underwater acoustic devices introduced early in the war both for offensive and defensive purposes.

Among these devices were submerged microphones at harbor entrances which could pick up propeller noises from any enemy ships or submarines which might attempt to sneak into the harbors under cover of fog or darkness. In all seas, our patrol vessels countered the submarine menace by scanning surrounding waters with rotating sonar beams. Inaudible to the human ear, these supersonic beams were reflected by submerged submarines which would thus unwittingly reveal their presence and location. On the offensive side, mines were laid on the bottom with mechanisms which would blow them up when the sound level from the propeller of a ship above it reached a sufficient magnitude.

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[*] Department of Geology, Columbia University, New York, N. Y. and U. S. Naval Ordnance Laboratory, Washington, D. C. This paper, accompanied by recordings, was presented before the 131st Annual meeting of the New York Academy of Sciences on December 15, 1948.

One of the earliest underwater sound installations introduced during the war was a hydrophone network set up off Fort Monroe to protect the entrance to Chesapeake Bay. During the late winter of 1942 the system worked very well indeed, yielding signals only when ships actually crossed the underwater array. But when spring came all this changed. Every evening the speakers on the surface would yield a babel of indiscriminate noises which resembled, more than anything else, the sound of a pneumatic drill tearing up pavement. The worst of it was that these extraneous noises masked the ship sounds completely and the whole system was useless for harbor protection. The army officers operating the system were baffled and called on the newly organized Office of Scientific Research and Development for help. A team of acoustics experts was rushed to the spot by the U. S. Navy Underwater Sound Laboratory at New London, Conn. They, too, were perplexed, but it did occur to one of them that fish might be responsible, and Dr. C. H. Blake, a marine biologist from M. I. T., was called in to investigate the possibility. Almost immediately, he found that the principal malefactors were indeed fish, specifically, large schools of croakers which had just come into the Bay after spawning in the open ocean. The acoustics experts measured the pitch of the fish noises and decided that they could be eliminated by a suitable electric filter which would cut out the fish noises, but let through the ship noises. After this was built and installed, no more fish were mistaken for ships, or vice versa.

About the same time engineers from the Naval Ordnance Laboratory in Washington were carrying on their own underwater listening tests at a nearby location in Chesapeake Bay. Their primary interest was in developing acoustic mine mechanisms. They too were bothered by the extraneous noises which had made so much trouble at Fort Monroe, and they were concerned by the implications of Dr. Blake's discovery that schools of fish could be responsible for louder noises under water than could be obtained from the propeller of a nearby ship. This meant that an acoustic mine might well be fired by fish long before a ship would ever come near. The Naval Ordnance Laboratory consequently initiated an extensive study of biological water noises that continued for nearly two years.

The first tests under this program gave some interesting information on the diurnal and seasonal variation of the fish noises in the Chesapeake. During the day, there was virtually no noise in the water, but with the coming of evening the sounds began and increased in intensity steadily until they reached their maximum loudness at about 8:30 P. M. After this time, they would fall off rapidly, until by midnight the noises were all but gone. The reason for this well defined evening crescendo and subsequent diminuendo, repeated night after night, is still not known, but they appear to have some relation to the croakers' feeding habits. Seasonal variations were not quite as smooth as the diurnal changes, but there were well defined trends. The nightly maxima, scarcely perceptible in early May, increased steadily until their peak value was reached about June 5. The intensities gradually fell off after this date until by mid-July the nightly choruses were no longer to be heard.

Meanwhile, on the west coast, a group from the U. S. Navy Radio and Sound Laboratory in San Diego ran into difficulties while testing high frequency sonar wave transmission. Where the ocean was deep, reflections were detectable from ships and submarines at much greater distances than in the shallow water with rocky bottoms characteristic of the California coast. Seeking the cause of this anomaly, the Navy engineers discovered that wherever the water was shallow a hydrophone would pick up an incessant noise similar to that of dry twigs crackling in a fire. This noise, by masking reflections from submarines, cut the range of sonar's effectiveness. Suspecting a biological origin, University of California marine zoologists under Prof. Martin Johnson caught specimens of every kind of marine life known to exist in the area, put them in aquarium tanks with hydrophones, and listened. After several months of failure, the culprit was discovered. It was the snapping shrimp, genus *Crangon*, a type of shrimp considerably different from the one we ordinarily eat. It is a tiny round crustacean less than half an inch in diameter with two claws of unequal size, the larger being the most conspicuous feature of its anatomy. By clicking together the two parts of this claw in the water, the creature makes an underwater pop comparable to the noise created when a swimmer clicks two stones together.

The total number of *Crangon* in the world must be a truly astronomic figure, since the crackling noises have been picked up wherever there have been underwater listening tests in marine waters less than 100 feet deep, between latitudes 35° N and 35° S.

As the war progressed, the tactical aspects of the biological noise problem changed somewhat. The Navy became more concerned with interference from the species inhabiting East Indian and Japanese waters than from those of Chesapeake Bay or San Diego Harbor. It was, of course, impracticable to record and measure noises from fish actually in strategic waters, therefore the next best solution was tried, namely listening to accessible fish resembling most closely those sonic species known to inhabit the seas surrounding enemy shores. Tests were accordingly conducted around Hawaii and the southwest Pacific islands, in home waters, and even in aquaria. Rather than to discuss all of these in detail, the author prefers to limit the remainder of his narrative to the small part of the total program with which he was personally involved.

The first of the projects in which he participated was in the world's largest aquarium, the John G. Shedd in Chicago. The Naval Ordnance Laboratory sent half a ton of sound equipment and two observers to this point, nearly 1,000 miles from salt water, to record the noises, if any, made by the marine specimens in captivity there. We managed to record some kind of noise from more than half the species in the aquarium, although some of these consisted only of crunching sounds at feeding time.

In the early summer of 1943, the Naval Ordnance Laboratory sent a party to the U. S. Fishery Biological Laboratory at Beaufort, N. C. with the principal mission of segregating various sonic fish species and recording the noises of each kind alone. The scheme was to send out fishermen who would bring back their catch alive, sorting out the different species into staked off enclosures in a salt water pond. An eavesdropping hydrophone would then be moved from one enclosure to another. Up until now, only the croaker had definitely been identified as a major contributor to the chorus of underwater noises picked up by hydrophones in east coast waters. Now we were trying to isolate the other culprits. Beaufort was chosen for this on account of its location near Cape Lookout, where the Gulf Stream leaves the coast line and starts across the Atlantic. At such a junction point, both tropical species brought up by the Stream and northern species live in close proximity.

Our first project at Beaufort was to listen to the normal water noises heard in Bogue Sound at that time of year, and this was done simply by hanging a hydrophone off the edge of the main pier on the Laboratory grounds. From the beginning, the variety and intensity of sounds was amazing. The drum rolls previously picked up in Chesapeake Bay from the croakers were heard, but, in addition, there were cackles, honks, crackles, and, most mysterious of all, periodic musical toots which resembled the sound of a fog horn and which occurred regularly about every five seconds, each toot lasting about a second. Sometimes, the toots would come in pairs, the second sounding as if it were in answer to the first. Additional listening posts were set up at Fort Macon on the inlet connecting Bogue Sound with the open sea, as well as on Coast Guard patrol boats which anchored in the ocean as much as 20 miles outside the inlet.

For our tests on the segregated fish, we staked off four enclosures with wire in an abandoned mullet pond and put representatives of a separate species in each. As tests were completed on a species, we netted out the specimens from its enclosure, replacing them with members of a new species.

About ten species made recordable noises. The croakers gave the familiar drum rolls, but with much less spirit and vehemence in captivity than when swimming freely in the waters outside. The black drum made a sound that was very much like that of the croaker. The sea robin, a red species looking almost as much like a bird as a fish, made a sound that resembled both a dog barking and a goose cackling. The spot gave intermittent honks that sounded something like "Bronx cheers." The hogfish grunted, just as one would expect from its name. The catfish produced a rhythmic drumming with the cadence of a tom-tom. The hardest noise to identify definitely was the strange musical toot, previously mentioned, which sounded like a fog horn and was heard almost incessantly around the Fishery Laboratories pier. The natives of the area said that the noise, which could be quite easily detected by anyone standing on the pier, came from the toadfish. But of the many toadfish we caught and transferred to the mullet pond, none would make any noise at all.

Then one day the loud speaker connected to the hydrophone off the Fishery pier began to give out a much louder "foghorn" toot than it ever had before at that gain setting and the needle of the recording meter jumped violently off-scale with each blast. Strangest of all, the source seemed to remain in one place for days, since the volume and cadence stayed the same for more than a week. The fact that the source apparently did not move suggested that it might indeed be a toadfish, since this species is a bottom dweller which builds nests in pipes or tin cans and remains in them for long periods. The identity of the noisemaker was finally established to our satisfaction, but in an indirect and circumstantial way. We lowered a baited crab trap into the water next to the hydrophone into which the loud "foghorn" noise was being emitted. A minute later, the noise stopped and the crab trap was pulled up with a toadfish inside. Yet, it was never possible to get a toadfish to sound off in captivity. Perhaps members of this species make noise only when guarding their eggs, the very high acoustic intensity they can develop paralyzing any intruder that ventures too near.

The mechanism by which many fish make noise under water was first discovered about forty years ago by R. W. Tower, who published a paper in the Annals of The New York Academy of Sciences in 1908 on noise production in the drumfish, toadfish, and sea robin. There seem to be two principal mechanisms. The first is stridulation or grinding of the pharyngeal teeth, which explains the noise production of the hogfish, grunt, and certain coral reef species associated with tropical waters. The more significant sonic species, however, make their noises through muscular action upon their swim bladders. The drumfish, or Sciaenidae, such as the croakers or black drums, strike so-called "drumming muscles" against their swim bladders about 15 times a second and set it into resonant vibration like a drum. As a specimen grows to full length, its resonant frequency decreases, since the length of the resonating swim bladder increases. Toadfish (Batrachoididae) and sea robins (Triglidae) are said to make their noise through stridulation of the swim bladders with muscles which rub them from the outside and give out a noise somewhat like that made when one strokes a wet balloon with his finger.

A question at least as interesting as *how* fish make noise is *why* fish make noise. Does their noisemaking activity perform some function associated with their mating or feeding habits? Do the fish communicate with one another and actually convey intelligence? What is the motivation? Having worked on the biological noise problem only as a physicist, the author does not feel qualified to suggest any answers to these questions. Perhaps the hydrophone may someday become a valuable tool to those studying behavior patterns in fish.

Another interesting question is whether there might be any post-war application of the wartime discoveries made in this field. There are at least two potential ones. The first is the possible use of acoustic methods to study fish populations and movements on a statistical basis. The federal government and many state biological surveys spend large sums annually for this kind of information and it may turn out that acoustic data can provide it at a minimum of expense. Only further research can establish whether the necessary correlations exist.

The other application would be to use sound equipment as a means of assisting fishermen in locating their catch. As pointed out earlier, this has ample precedent dating back to ancient times, but it does not seem to be entirely promising. In the first place, many food fish are not sonic, the great majority north of Chesapeake Bay being quite silent. Secondly, most sound producing fish are noisy only for a few hours a day, usually at night. In the third place, it would be impossible to hear any fish as long as the fishing vessels' propellers are turning, a factor that would restrict the mobility of such fishing operations. Finally, the complexity of the present equipment would limit its acacceptance by most fishermen. Nevertheless, some manufacturers of marine equipment are considering the marketing of underwater listening kits for fishermen. Perhaps the various difficulties can be ironed out through further development and, in a matter of a few years, we will find most fisherman wearing earphones at their work. If this should be the case, it would not be the only instance of research instigated purely by wartime necessity leading to useful and profitable peacetime applications.

Abstract: EFFECT OF CONCENTRATION ON THE VISCOSITY OF DILUTE SOLUTIONS

By DOCTOR ROBERT SIMHA*

Awarded an A. Cressy Morrison Prize, 1948

Doctor Simha's paper sets up and solves an usually difficult problem in theoretical hydrodynamics. Considering the necessary approximations, his predicted result is in good agreement with the experimental values, and we now will have a theoretical explanation of a property of solutions which has been a puzzle for a long time.

Viscosity, in Physics, is the resistance of a fluid to the movement of its molecules among themselves. These particles may be spherical but, more likely, non-spherical, and the resistance and flow of the fluid is dependent upon their shape and concentration, i.e., their nearness to each other.

The author presents a theory, represented by a power series formula, of the higher coefficients of the viscosity-concentration curve of a suspension of non-spherical particles. Starting with a simple model of dumb-bell, to represent such a non-spherical particle, the flow around a single unit and its modification due to interaction with other particles are considered.

In this paper, it is the purpose of the author, with reference to the formula above mentioned, to examine the validity of the proportional increase in complexity of the terms of the equation as expressions of the increasing complexity of the reactions of progressively increasingly numbers of particles of varying shape in their disturbance of the flow of the liquid, thus forming a measure of the intrinsic viscosity of the fluid.

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* National Bureau of Standards, Washington, D. C.

Abstract: THE RELATION OF THE ADRENAL CORTEX TO THE STRUCTURE AND PHAGOCYTIC ACTIVITY OF THE MACROPHAGIC SYSTEM

By DOCTOR ALBERT S. GORDON AND MRS. GRACE F. KATSH*

Awarded an A. Cressy Morrison Prize, 1948

There is considerable evidence that the endocrine gland system plays a role in the response of the body to a wide variety of physical, chemical and microbic stresses. Hormonal administration or deficiency cause alterations in the quantities of circulating antibodies. The lymphatic system and the macrophagic tissues are implicated, as is well-known. The influence of ductless glands, however, upon the phenomenon of phagocytosis has not been thoroughly explored.

During the past two years, the authors have conducted a series of experiments on male rats to examine the influence of the adrenal cortex and the pituitary upon the structure and functional activity of the macrophagic system, through chemical analysis, histological studies and macrophagic cell counts, with especial reference to the spleen, the Kupffer cells of the liver, the lymph nodes,' thymus and bone marrow. Removal of the adrenals and pituitary affects the production of phagocytes by the tissues of these organs.

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* Department of Biology, Washington Square College of Arts and Sciences, New York University, New York City.

Abstract: PORTO RICAN PREHISTORY (In Two Parts)

By DOCTOR IRVING ROUSE*

Awarded an A. Cressy Morrison Prize, 1948

This paper is the result of three seasons' research on the prehistoric native inhabitants of Porto Rico and their culture, as deduced from an extensive series of excavations in all parts of that Island, by the author. The pottery unearthed, much of it in fragments, in the light of the successive layers in which it occurred and the geographical distribution of the various types into which it was carefully classified, shows that there were three prehistoric races that successively occupied Porto Rico before its discovery by the Spaniards.

The first, a primitive people, the Ciboney, migrated to Porto Rico from Florida. Their remains, indicated only by bone, rough stone, and shell implements, show that they were a non-agricultural race. They were overcome by the Arawaks, South American Indians, who entered the West Indies by way of the Lesser Antilles and the Virgin Islands. They seized possession of Porto Rico, it is believed, about the year 929 A.D., and developed an agricultural era. Their remains are abundant in the form of pottery of characteristic types which fall into two successive categories. About 1437, they were displaced by the warlike Caribs, who inhabited the region when the Spaniards came, and have left remains throughout Porto Rico in the form of later types of pottery.

Doctor Rouse's paper analyzes the evidences for this succession of cultures, and their chronological implications, with scholarly insight.

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* Associate Curator, Peabody Museum of Natural History, Yale University, New Haven Connecticut.

Abstract: DRAINAGE EVOLUTION IN THE APPALACHIANS OF PENNSYLVANIA

By DOCTOR HENRY D. THOMPSON*

Awarded, the George Frederick Kunz Prize in Geology and Mineralogy, 1948

Geologists, generally agreed that the watershed between the eastward and westward flowing streams in the Pennsylvania region, before and during Upper Triassic time was far eastward of its present position, with the land much higher that at present. In fact, the main divide is now beyond the Ridge and Valley province, eighty miles or more from the crystalline rocks where it was formerly located.

During the earlier period of the elevated continent, the streams flowing eastward to the Atlantic, though much shorter than those on the west side of the divide, were much steeper in their course to the sea, and with much greater cutting power.

Doctor Thompson utilizes this feature as the beginning of his explanation of the remarkable reversal that took place in the courses of many of the streams. He states that "over a broad belt, the streams that formerly ran westward now flow eastward. How did this complete reversal take place?" Doctor Thompson argues that the superior cutting power of the steeper eastward flowing streams of the period enabled them to cut backward toward their sources, forming deepening canyons toward their head-waters and weakening the ridge of the divide by erosion, thus forcing it westward. At the same time, it invaded the head-waters of the westerly flowing streams "capturing" them and reversing their flow.

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* Professor of Geology and Head of Department, Hunter College, New York City.

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TRANSACTIONS
OF
THE NEW YORK ACADEMY OF SCIENCES

Editor
ROY WALDO MINER

Associate Editor
PAUL LENIHAN

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Executive Director: EUNICE THOMAS MINER

THE ELECTRON MICROSCOPE APPLIED TO GEOLOGICAL RESEARCH

By THOMAS F. BATES*

Introduction. The history of microscopy, as applied to the geological sciences, begins in the 1820's with William Nicol, who devised the polarizing device that bears his name[1] and who also described a method of making thin plates of fossil plants.[2] The investigation of thin sections under plain and polarized light was, however, largely a curiosity until the work of H. C. Sorby. In 1859, this scientist, speaking before the Geological Society of France, said:[3] "The study of thin plates of rocks constitutes an entirely distinct branch of geology and demands additional knowledge. In the ten years since I began this, I have prepared many hundreds of these plates, and nevertheless much remains to be done. If I wished to describe all the results to which their examination led, I should have to go into each branch of geology;..." Even such a far-sighted scientist as Sorby, however, did not conceive of the tremendous changes in geological thought which were to be produced by this tool in the hands of men such as Zirkel, Rosenbusch, Fouqué, and many others.

Ten years have elapsed since the electron microscope was first applied to geological problems. During this decade, little progress along geological lines has been made compared to that in such fields as bacteriology and metallurgy. Nevertheless, this tool is certain to play an important part in future geological research, and though it may not influence geological thought as greatly as did the petrographic microscope, it will provide the key to the solution of many geological problems.

Advantages and Limitations of the Electron Microscope. In order fully to appreciate the significance of the electron microscope in geological research, it is important to be aware of the advantages and limitations of the instrument. The most important advantage is the increase of resolution which results when an electron rather than a light beam is used. Whereas the light microscope can only distinguish points as individuals when they are 2,000 angstrom units (0.2 microns) apart, the electron microscope effectively separates in the image two points which are only 10 angstrom units distant in the object.

On the other side of the ledger, the most important limitation in the use of the electron microscope in geology is the low penetrating power of the electron beam. PLATE 1 (A) shows a film of collodion which has broken and doubled over upon itself. Although the film is 1,000 times thinner than a standard thin section, three thicknesses are almost opaque. If this material had a greater density—as for example, that of a sheet of mica—it would have to be still thinner to be translucent.

PLATE 1 (B) of diatomaceous earth shows the varying degree of translucence in different portions of the fossil organism. It is apparent that transmission studies with the electron microscope of so-called "thin sections" of rocks and minerals is not practical at the present time. Only in the case of platy or fibrous particles is it likely that the electron beam will penetrate the material.

A second limitation arises from the fact that the electron beam operates only in a high vacuum and generates heat in a particle in its path. This is particularly serious in bacteriological work but also affects certain applications to geology.

Finally, it is important to realize that color, birefringence, refractive index, and other optical characteristics which make the petrographic microscope such a valuable tool, have no significance when dealing with electron optics.

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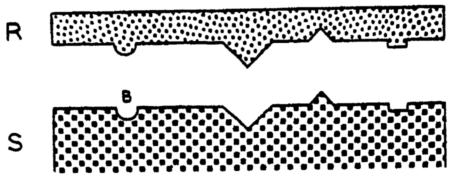
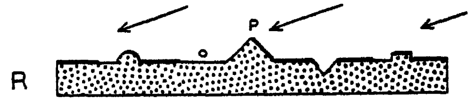
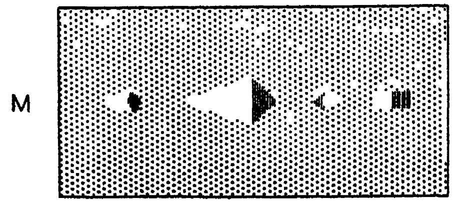
Despite the serious nature of these limitations, they can frequently be circumvented. FIGURE 1 illustrates the ingenious method used to overcome difficulties imposed by the low penetrating power of the electron beam. Imagine that we are interested in studying the crystals and bubbles in a piece of obsidian. A cross section of the fragment might resemble section S where B is the hole left by a bubble, the elevation represents an octahedral crystal projecting from the surface, and the depressions are left by crystals projecting from the fragment that has broken off. Since it is impossible to get an electron beam through "S," a replica "R" is made from it in the form of a very thin film of collodion. If the film is removed and placed in the microscope, the variation of thickness in the film will be portrayed by variation in density in the electron micrograph. Since this variation is rather slight, another technique is used to increase the contrast. The replica is placed in a vacuum and a metal, commonly chromium or gold, is evaporated so that it strikes the surface at a low angle—here 20 degrees. The metal piles up on one side of a projection and is absent in the lee of it. In the figure, point "o" is in the shadow of projection "p." When the electron beam now impinges on the shadowed replica, the contrast is sharply increased.



Replica (R) from Sample (S)



Metal shadowing of replica



Micrograph of shadowed replica

FIGURE 1. Steps in the preparation of a negative replica

Shadowing also provides a means of measuring accurately the third dimension. In the case of projection "p," if the shadow cast is one micron long and the angle 20 degrees, a simple calculation shows that the crystal projects 0.3 microns above the surface.

Shadowing is also useful where objects are viewed not in replica, but directly. This is shown in PLATE 1 (C) and (D), showing particles of kaolinite from Zettlitz. PLATE 1 (C) without shadowing, shows the characteristic morphology, and by variation in density gives an indication of thickness. Some of the thinnest flakes are difficult to distinguish from the background.

In the shadowed specimen, PLATE 1 (D), the contrast is increased. In this case, the chromium impinged on the upper right of each particle or aggregate and is absent in the shadow on the lower left. Knowing the angle of shadowing to be 15 degrees, it is possible to measure the length of the shadow and ascertain that flake "A," for example, is 120 angstrom units in thickness. Flake "B" is 80 angstroms and, therefore, about 11 unit cells in height.

This illustrates how some of the limitations may be overcome, and shows why almost 75 per cent of the time spent in electron microscopy is devoted to sample preparation.

Application to Geological Problems. There are now hundreds of electron microscopes in research laboratories throughout the country, but, unfortunately, very few are used primarily for the study of geological subjects. This is a deplorable situation and one which should soon be remedied. As this paper will indicate, there is a large amount of work to be accomplished in the high magnification phase of geological research.

Clays have been studied with the electron microscope more than any other mineral group. For one reason, the interest in these minerals has increased tremendously in the last decade; second, their size range is about right for electron microscopy; and third, samples are easily prepared and replica techniques not necessary.

Micrographs (C), (D), (E), and (F) in PLATE 1, illustrate the utility of the electron microscope in detailed morphological studies of these fine-grained minerals. Pictures (C) and (D) show the hexagonal flakes typical of kaolinite. Illustrations (E) and (F) are of halloysite, a mineral of structure and composition similar to kaolinite, but with a notably different habit. Although halloysite crystals are usually described as lathlike, our electron micrographs, such as in PLATE 1 (E), show that the forms are hollow tubes, "a," which are commonly split as in "b," which have collapsed. Double tubes are also frequently seen, as at "c" and "d." PLATE 1 (F) shows circular cross sections of similar tubes.

A story of important geological significance is told in electron micrographs (A) and (B), PLATE 2. An investigation of the micaceous minerals in the slate of northeastern Pennsylvania[4] revealed that the most abundant of these is a mineral of the hydromica or illite group. Since it is to this mineral that the slate owes its cleavage, its identity was carefully checked by differential thermal analyses, firing tests, optical data, and x-ray patterns. All of these showed that the material from the slate behaved exactly like the illite from shale that Dr. Grim has described.[5] However, a striking difference is observed in the micrograph of the illite from shale, A, and that from the slate, B. It is apparent that the very fine shreds of illite in the shale in Pennsylvania were recrystallized into much larger shreds and flakes and oriented in parallel alignment to give rise to slaty cleavage. An electron microscope study of a series of samples across the Appalachian structure, from shale to its metamorphosed equivalent, might show how the crystallinity of the fine micaceous fraction changes. Such a study would possibly shed much light upon the nature and degree of metamorphism from place to place throughout the Appalachians and other folded regions.

Micrograph (C), PLATE 2, is of interest in connection with (A) and (B), PLATE 2. From other data this mineral also appears to be an illite. It is the material which forms the Ordovician metabentonites near State College, Pennsylvania. A detailed morphological study of the illites and montmorillonites is now underway.

These two problems illustrate the study of particles under the electron microscope. The other micrographs illustrate the possibilities of the replica method. For comparison of the two methods, micrographs (D) and (E), PLATE 2, are of interest. In a study of asterism in phlogopite mica, straight transmission work proved to be ineffective. In ground flakes of the mica ((D), PLATE 2), there is some evidence of needles, but they are not symmetrically arranged and Fresnel diffraction bands obscure much of the detail. Consequently, a fresh cleavage was obtained, a drop of collodion allowed to run down over the surface and dry, the replica was removed mechanically, shadowed, photographed, and is seen in (E), PLATE 2. The inclusions are readily visible and the angle between them is exactly sixty degrees. Even many of the tiny dots fall into line. It is noticeable that some of the needles are unusually large and that material which normally would form many minute needles has gone into their formation, leaving the surrounding area smooth and devoid of inclusions. Probably such a phenomenon accounts for the common zones in phlogopite which do not show asterism. The area in the upper left corner bordering the large crystals would be such a zone on a very small scale.

Electron micrographs (F), PLATE 2, and

(A) and (B), PLATE 3, illustrate a study of crystals and bubbles in opal glass.[6] They are included here because similar studies on natural and artificial glass are of mineralogical and geological significance.

PLATE 2, (F), represents the broken surface of a typical glass containing fluorite as the solid phase. The crystals are well developed octahedrons or cubes, or combinations of both. The streak in the matrix appearing on the same side of each crystal results from breakage of the glass.

PLATE 3, (A), illustrates a sodium fluoride glass which shows a characteristic grouping of crystals and "globules" in a matrix dotted with bubbles.

The dendrites shown in PLATE 3 (B) are common in many of the glasses studied. Several features are of interest. Bubbles are found in the dendrites at the tips of the "branches." The fracture of the glass in the vicinity of the dendrites is smoother than that of the matrix in general. In several cases, the branches terminate in four-sided polygons of distinctive shape.

The same technique used on obsidian (PLATE 3 (C)) reveals at high magnification some of the crystallites which, in the form of feathery shadows, have long been observed in the light microscope. Work of this kind would be particularly significant from the standpoint of the investigation of devitrification and crystal growth.

The remaining electron micrographs show other subjects of general geological interest. To investigate carefully the possibilities suggested by each would require a large amount of careful work with both the light and electron microscopes. PLATE 3, (D), pictures the structure of calcite on a polished and etched longitudinal section of Belemnites. It is quite possible that much of the detail was produced during polishing of the section.

PLATE 3 (E) is a micrograph obtained from a fractured surface of milky quartz. The significance of the structure is unknown but there are many like it in the specimen. No bubbles were observed.

PLATE 3 (F) illustrates a replica of the face of a smoky quartz crystal after light etch with hydrofluoric acid. A growth line on such a face becomes a growth "zone" under the electron microscope. The structure brought out by the etching is suggestive of a very fine-grained aggregate. A careful study combining electron microscope and electron diffraction techniques might shed much light on the problem of the smoky color in this type of quartz.

PLATE 4, (A), (B), (C), (D), constitutes a series of electron micrographs of a fractured surface of novaculite, the fine-grained silica rock. The initial magnifications in the electron microscope were 2,000, 4,500, 7,000, and 10,500×. The siliceous ooze from which this quartz is formed apparently crystallizes into particles which in some cases appear to have crystal faces.

These micrographs illustrate a few of the possible applications of the electron microscope to geological research. It is apparent that this instrument will open many avenues of research that have heretofore been closed. Sorby was commonly faced with the criticism that it was impossible to look at a mountain through a microscope. The electron microscope has made the mountain 100 times bigger, but just as the light microscope has become standard equipment in all phases of geology, so the electron microscope will soon be taken for granted as a powerful geological research instrument.

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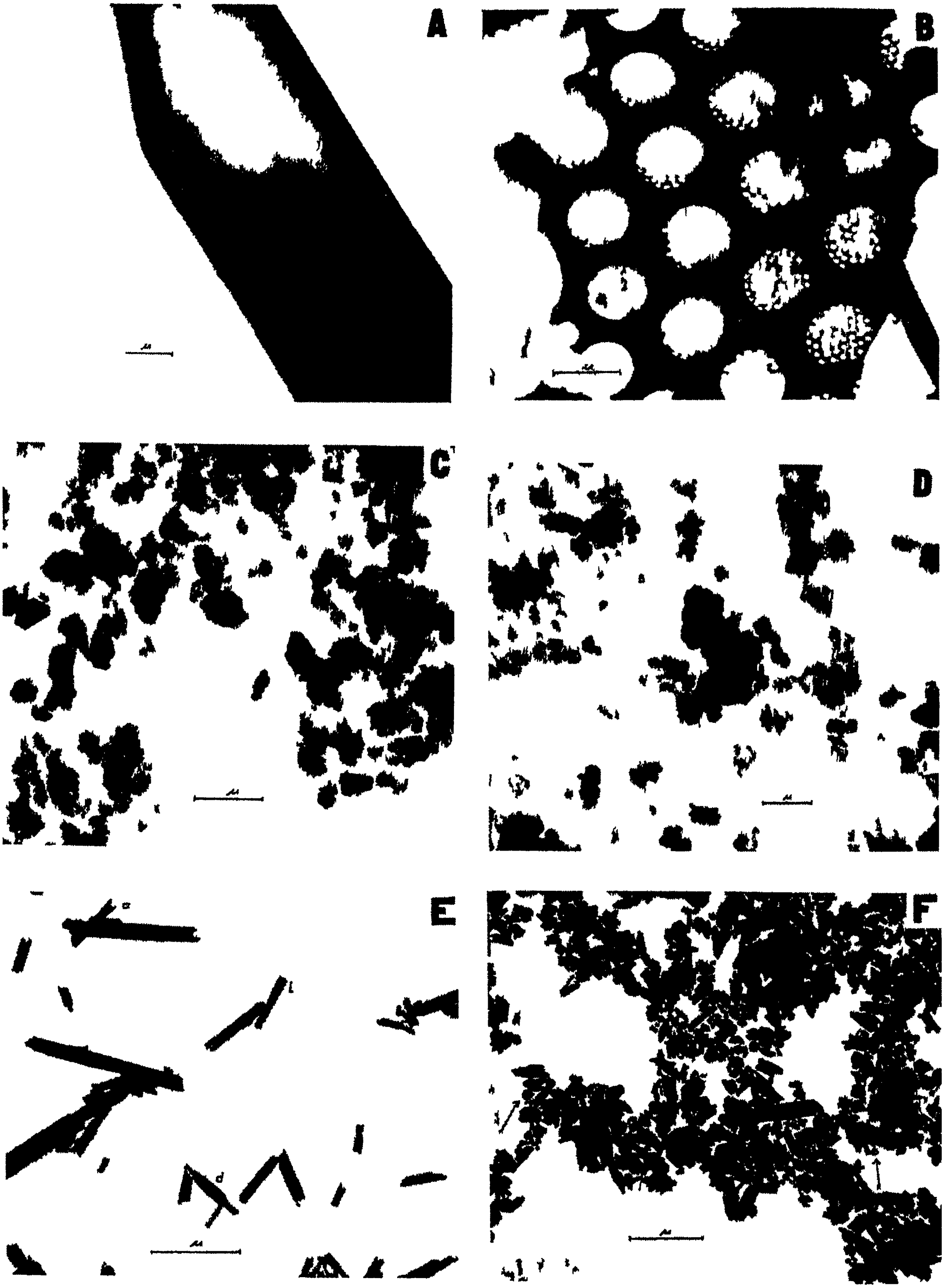

Plate 1

A Collodion film
C Kaolinite unshadowed, Zettlitz, Austria
E Halloysite, Real County, Texas

B Diatomaceous earth Santa Barbara County, California
D Kaolinite, shadowed Zettlitz, Austria
F Halloysite Beuthen Silesia

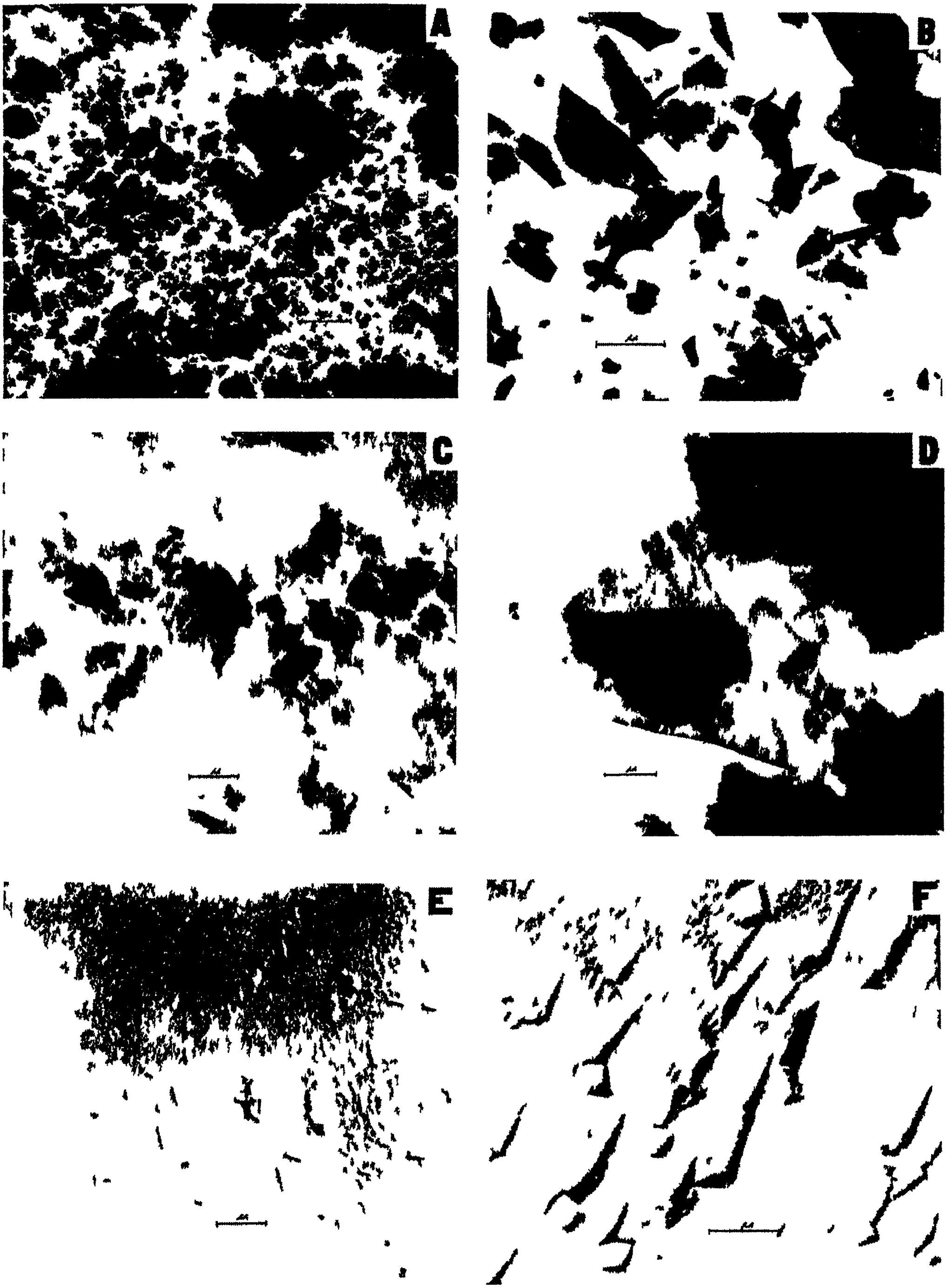

PLATE 2

A Illite from shale Fithian Illinois
C Metabentonite mineral Lemont Pennsylvania
E Replica of phlogopite mica surface Quebec Canada

B Illite from slate Pen Argyl Pennsylvania
D Phlogopite mica particles Quebec Canada
F Fractured surface of fluorite opal glass

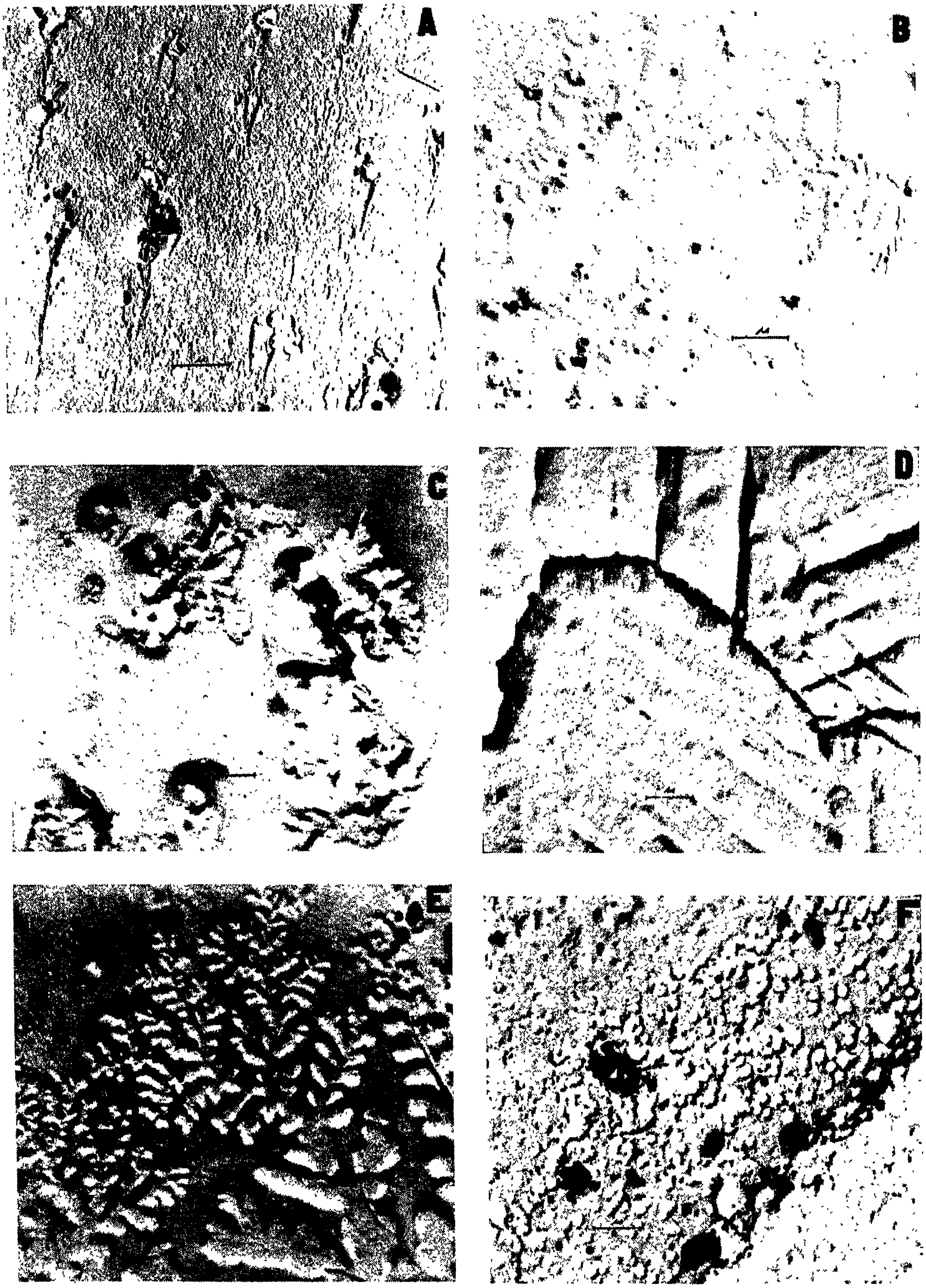

PLATE 3.

A. Fractured surface of sodium fluoride opal glass.
C. Crystallites in obsidian; Bombardos, Greece.
E. Fractured surface of milky quartz; Dillsburg, Pennsylvania.
B. Dendrites in opal glass.
D. Calcite structure in longitudinal section of Belemnites.
F. Crystal face of smoky quartz etched lightly with HF; St. Gotthard, Switzerland.

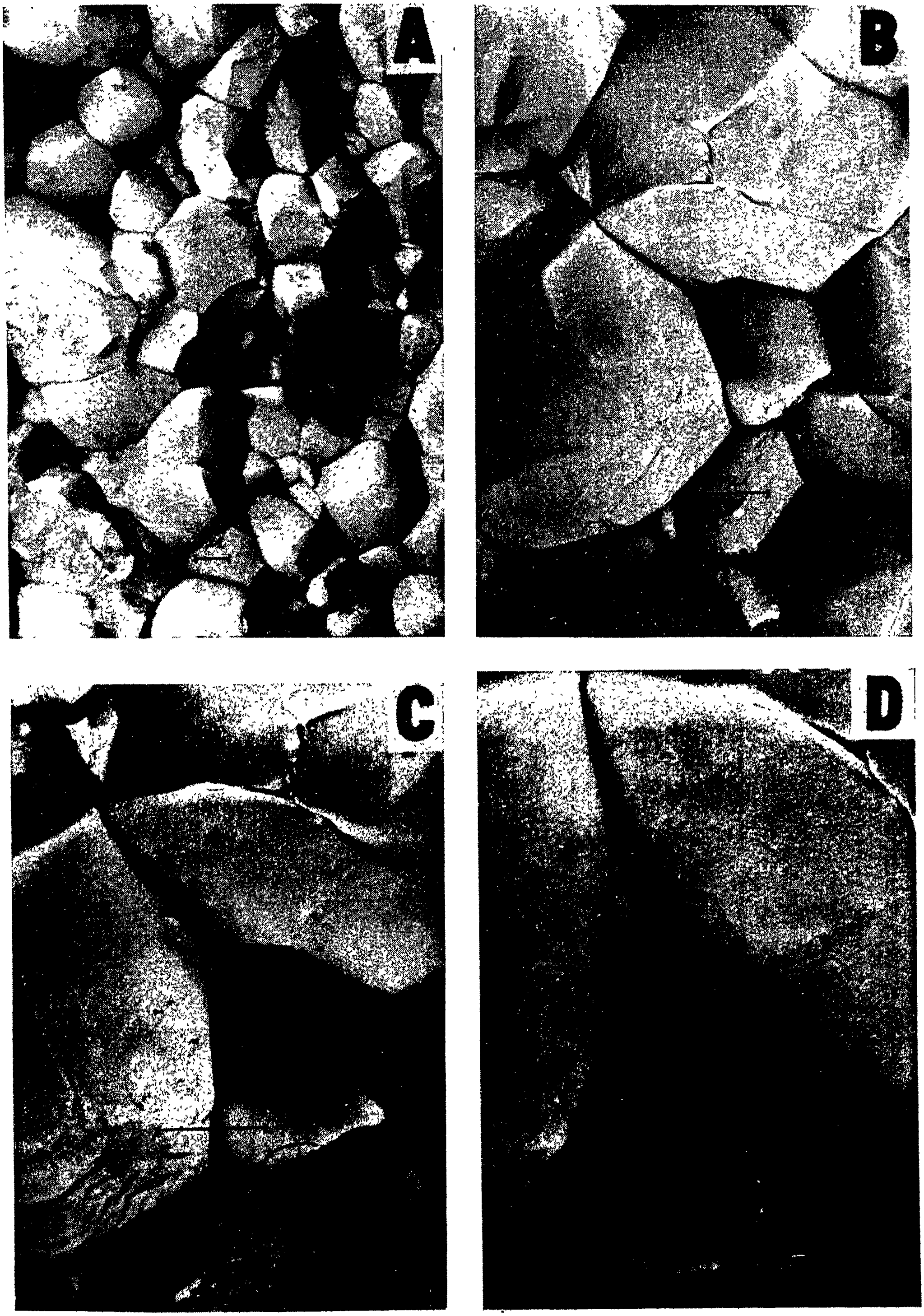

PLATE 4.
Fractured surface of novaculite; Hot Springs, Arkansas

SECTION OF BIOLOGY

A.T.P.-ASE ACTIVITY IN EMBRYONIC DEVELOPMENT

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The basic problem which is considered in this paper is the problem of differentiation. There are two aspects of this problem. First of all, an external stimulus is necessary to convert an undifferentiated cell into a differentiated cell. This can be done in a variety of ways. In studies on the determination of the polarity in hydroids, it is found that polarity may be determined by imposing a hydrogen ion concentration gradient across a stem of the hydroid. Similarly, in the egg of *Fucus* (Whittaker), the polarity of the egg can be determined either by hydrogen ion concentration, by an electrical current, or by light. Another type of external stimulus that will determine differentiation is that resulting from embryonic induction. The induction of the neural plate can be brought about by a variety of artificial agents. The substitution of various dead tissues for the amphibian organizer falls into this classification. Here, too, must come the various carcinogenic agents which when applied to skin will cause cancer. These are all external stimuli which change the characteristics of the cell so that it becomes different from its previous state.

The second aspect of differentiation is an internal change in chemical composition. This results from the external stimulus. We would like to know what is the first indication, the first change in the cell after the stimulus is applied. An internal chemical change is inferred from embryological work. We know that before there is any visible differentiation in a cell, it has acquired chemodifferentiation. This can be shown by transplantation and the resultant self-differentiation of the cells in a foreign environment.

What is the general nature of this internal change, this chemodifferentiation? In the first place, it must be an irreversible change. Once the amphibian organizer has induced the ectoderm toward the formation of a neural plate, then that ectoderm may be removed from the action of the organizer and it will go on and form a neural tube. It can be exposed to other inductors and will no longer respond. Therefore, this change, to some extent at least, is irreversible.

The second aspect of this internal change is that it must be an internal rearrangement of the chemical compounds in the cell, because nothing new is added when the cell differentiates. The evidence for this statement is indirect and resides in the fact that a variety of chemical compounds, many of them not normal cell constituents, will induce a neural plate in the amphibian ectoderm. An abnormal cell constituent cannot persist in the cell and therefore must act by producing a rearrangement of compounds already present.

To go on with this background, the rearrangement of these cellular constituents during the early phase of differentiation could be release of a compound from an inactive state. Thus, substances responsible for neural differentiation may possibly be bound in an inactive form in the ectoderm. The organizer would then be an agent which liberates this compound. The initial change could also be the formation of a new compound by a regrouping of components of the cell. A protein or enzyme might adsorb another ion or molecule and, thus, form a complex which may have new properties.

A third possibility is that a shift in equilibrium among the various components of the cell may result in an increase in the concentration of the compound which is responsible for differentiation. This would call for a picture of the undifferentiated ectoderm as containing all the compounds for the differentiation of nerve, epidermis, notochord, muscle, *etc.* These substances would be in an equilibrium such that no one predominates. When the neural inductor stimulates ectoderm, then the chemical reactions giving rise to neural substances are favored and the rest break down and are converted to neural substances. A competition between various reactions may result in a suppression of all the modes of differentiation except one. An example of competition may be taken from experiments on the hydroids. Both ends of the stem of a hydroid *can* form hydranths. In terms of differentiation substances, all the substances are present. Only one end *does* form a hydranth, however, when the two ends are in competition with each other. One end forms a hydranth and the other end a stolon. Both ends can form hydranths if we interpose a ligature between the two ends. If this type of competition can be extended to reactions within a cell, then a cell may be able to form a neural, epidermal or muscle cell, but if an external substance which will favor one reaction as opposed to another is added, then a shift in one direction would occur.

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All this leads, of course, to the supposition of differentiation substances responsible for the differentiation of the cell, and in any case, regardless of their origin, we want to know what these substances are like. The substance must be self-duplicating in order to maintain the cell type under a variety of external conditions. A self-duplicating unit of some sort must form in the cytoplasm which will continue to duplicate itself more or less independently of the environment. If these substances are self-duplicating, then they need an energy source. The ultimate source of energy must reside in cellular oxidations. And, finally, there must be some linkage between cellular oxidations and the self-duplicating substances—a transfer of energy between the actual oxidation in the cell and the self-duplication of the compound within the cell.

Turning to specific examples, we ask what the energy source is in the case of the frog egg. It has been known for many years that glycogen disappears during early embryonic development in the frog egg. The steps in the breakdown of glycogen appear to be very similar to the steps in glycogen breakdown in muscle. In the first place, a triose-phosphate-dehydrogenase is present in the frog egg, as shown by Brachet and Rapkine. In the second place, lactic acid is formed under anaerobic conditions and development takes place at the expense of lactic acid formation and an oxygen debt. Third, phosphocreatine is present. These studies, in addition to some work dealing with a block to respiration in hybrids, led us to the general idea that perhaps the same type of phosphorylating glycolysis is present in the frog egg as in muscle, in which case the terminal step would be the formation of A.T.P.

Studies on phosphorylation in the frog egg showed that the A.T.P. concentration does not change very much with development. That is, the absolute amount of A.T.P. during cleavage, gastrulation and neurulation is about the same. This situation might be expected if A.T.P. were part of a system for the transfer of energy from the oxidations to some acceptor in the cell. If the frog egg is placed under anaerobic conditions, then the A.T.P. breaks down and inorganic phosphate appears. Under these conditions, the gastrula continues to develop with the concentration of A.T.P. dropping from about 25 gamma per 100 eggs to about 16 gamma. Now, this breakdown of A.T.P. under anaerobic conditions probably also occurs under aerobic conditions; but, with the cellular oxidations intact, the A.T.P. is constantly resynthesized so that its concentration does not change. The resynthesis of A.T.P. can be shown by first exposing the eggs to anaerobic conditions, where the A.T.P. breaks down, and then readmitting oxygen into the system, in which case the A.T.P. is then built up again to its normal concentration. This resynthesis of A.T.P. after breakdown is very rapid. After about 22 hours exposure to nitrogen, during which time A.T.P. breaks down, recovery occurs within about two hours.

Thus, the development of the egg under anaerobic conditions appears to be at the expense of breakdown of A.T.P. and, when oxygen is readmitted, the A.T.P. is synthesized again to its normal level. Now, if A.T.P. is the link between respiration and development, there must be some means of shunting the energy from A.T.P. to the various processes of differentiation. There must also be various acceptors of the energy-rich phosphate. And, thus, we look for a group of enzymes which will release and transfer phosphate from A.T.P. to the various acceptors in the cell. These acceptors would then be the differentiation substances which we assumed earlier in this discussion. On the basis of economy of hypothetical substances, we might assume that the enzymes which act on A.T.P. are the actual differentiation substances themselves. That is, the enzyme which breaks down A.T.P. may also accept the phosphate and in so doing perform work in the cell. This situation is not without precedent, because a similar situation was formerly postulated for muscle. Myosin was considered to be A.T.P.-ase. In a sense, that is still true. However, myosin turns out to be a rather firm complex of several proteins; and perhaps differentiation substances within the cell may turn out to be a complex of several proteins which act as a unit. In any case, the role of enzymes which act on A.T.P. would appear to be important.

Following the procedures worked out for muscle, protein extracts which liberated inorganic phosphate from A.T.P. were obtained. Since a water-soluble enzyme from potato had been reported, we tested water extracts from the egg and found high A.T.P.-ase activity in water as well as myosin extracts. Finally, extracts were made in 10 per cent NaCl at pH 6.9 because under these conditions myosin was reported to be relatively insoluble. We wanted to see what the A.T.P.-ase activity was in an extract that does not contain myosin. This gives us three protein fractions, each of which yields A.T.P.-ase activity. These three extracts have different properties.

If we plot the activity of the extracts during cleavage, gastrulation, and neurulation, the activity of all three extracts is low during cleavage. The water extract shows an increase in activity up to gastrulation and it begins to fall off after gastrulation. The 10 per cent NaCl extract starts low and increases, but does not fall off during the interval from gastrula to neurula as does the water extract. The myosin-like extract starts off rather low and is always lower than the water and 10 per cent NaCl extracts.

These three extracts differ in their activity at different hydrogen-ion concentrations. The water extract shows a regular increase in activity with increase in pH and is very high around pH 9.0. The 10 per cent NaCl extract shows a more complicated pH curve with an optimum at pH 7.0 and a second one in the alkaline range, around pH 8.8–9.0. Thus, there are two peaks of activity in this extract. The activity of the KCl extract increases as the solution becomes more alkaline.

The activity, with respect to time of incubation, shows the water extractable enzyme to be a very stable system which does not fall off after an hour of incubation. Some of the extracts from mammalian tissues show a very sharp decrease in activity with respect to time. The 10 per cent NaCl extract begins to fall off after about 10 to 15 minutes, and the KCl extract falls off very rapidly with respect to time. The latter is the most sensitive A.T.P.-ase of the three, with respect to temperature.

From these observations, three different A.T.P.-ases were indicated to be present in the frog egg. Of course, there is always the possibility of a single enzyme combined with different proteins, and, therefore, having some different properties. Indeed, some studies of these three fractions from adult frog tissues showed that the NaCl and KCl extracts possessed identical pH curves. That is, in the adult, the A.T.P.-ase activity, with respect to pH, is low at pH 5.0, has one optimum at about 6.0, another at about 7.0, and then a much higher optimum at 8.8–9.0. The KCl extract showed the same optima but the curve was shifted somewhat. Thus, in the adult, the extract with 10 per cent NaCl and 0.5 M KCl seemed to be identical. The water extract, however, shows a quite different curve with an optimum at about 7.0–7.2.

It is interesting that the A.T.P.-ases in the adult show different characteristics from those of the embryo. The pH curve of the NaCl fraction in the embryo has an optimum at 7.0 and another at pH 9.0. Two of the three optima of the adult are present, but the third is lacking. It would look as if during embryonic development the 10 per cent fraction acquired another optimum at pH 6. Some change must have occurred during development. That change occurs in the early tadpole. In the tadpole, the optimum at pH 6 is present. Another difference between embryo and adult extracts is in the pH characteristics of the water extract of the embryo, which is low at pH 5.0, and increases rather steadily to pH 9. Whereas, in the adult, the water extract starts low, reaches a maximum at pH 7.0, then falls off. Evidently, we are dealing with different A T.P.-ases in the embryo and adult.

Considering the evidence so far, the work on the frog indicates that A.T.P.-ases play a role in development as a whole. A.T.P. is necessary for development under anaerobic conditions, A.T.P.-ases are found during development, and they increase in amount. Thus, there is some relationship between A.T.P. and development as a whole. But in development of the whole egg, many processes occur besides differentiation—cell migrations and foldings of layers, in which physical work is done. There is also a general growth process, a synthesis of new protoplasm taking place when an egg develops, so that we cannot relate these A.T.P.-ases directly to cellular differentiation. We look, therefore, for a system in which growth and cell migration are lacking and try to find one in which differentiation predominates.

This situation was found in a colonial ascidian at Bermuda. This ascidian reproduces asexually by budding from a stolon such that at fairly regular intervals a zooid forms. If the stolon is cut at any level and a piece removed, an outgrowth develops from each end. Buds form and differentiate into zooids. This differentiation of the stolon into zooid can be controlled.

In the first place, these pieces of stolon exhibit what is known as a group effect, described by Whittaker for the egg of *Fucus*. This is a phenomenon in which several individuals placed in close proximity, in a geometrical pattern something like spokes of a wheel, have a mutual effect on each other. The stolon grows outward from all ends, but the buds and young zooids only form from the peripheral ends. Thus, a mutual inhibition of the ends which are in close proximity is obtained. The stolons migrate and fuse to form a complicated network within which the circulations of the individuals intermingle freely. Something accumulates in the central area which prevents the formation of zooids, without however, interfering with the development of the stolon.

The simplest assumption is that excretory products—carbon dioxide, urea, uric acid—accumulate at this point and inhibit development of the zooids. These three agents were tested in a gradient chamber which consisted of a reservoir for the agent and a large surrounding free space. From this reservoir carbon dioxide, for example, diffused and came in contact with one end of each of these stolons, and the other end was bathed by sea water. Under these conditions, stolons will grow from the end exposed to carbon dioxide, but the zooids form only from the free ends.

Thus, the stolon represents a mass of undifferentiated cells, in the sense that any group of cells within the stolon may form a zooid. In addition, differentiation can be controlled by an external chemical stimulus. The equilibrium between the zooid and the stolon can also be controlled by temperature. If a zooid attached to a stolon is placed at 28°C., the cells from the zooid break off from the various organs and accumulate in the stolon. The stolon becomes very thick and packed with cells. The phenomenon has been called regression and was discovered very early by Driesch. It was re-examined by Huxley and recently by Deviney.

If the stolon is now transferred to 22°C., the process will reverse and a zooid will differentiate from the cells of the stolon. The system can undergo regression, in which all morphological differentiation is lost, and then can undergo differentiation once more. The cycle can be repeated several times.

How does the A.T.P.-ase activity correlate with this regression and differentiation? Here no growth processes occur to speak of, no synthesis of new protoplasm. The cells are already formed. For the most part, there seems to be a change from a spherical, undifferentiated cell in the stolon to the differentiated cell in the organs of the zooid. Zooids were cut off from the stolons and investigated for A.T.P.-ase activity. The water extract yields high A.T.P.-ase activity from the differentiated zooid. From the stolon, however, little or no measurable A.T.P.-ase activity was found. If we take these stolons and put them at 22°C., as soon as the buds come out, the A.T.P.-ase activity begins to shoot up very sharply. The A.T.P.-ase activity of water extracts was measured at different times, beginning with the completely regressed zooid in the stolon and during bud formation and differentiation of the buds into a zooid. The activity is very low at first, and then shoots up very sharply. The first change in the stolon is the formation of outgrowths from the two ends. These advance along the bottom of the dish. When spherical buds form at both ends the A.T.P.-ase activity rises sharply.

Thus, we find that the A.T.P.-ase activity disappears when the zooids lose their organization and reappears when they reorganize into a new zooid. A structural dedifferentiation is accompanied by a biochemical dedifferentiation.

Since the formation of the zooid can be controlled to some extent by external factors, it brings up the question of the possible correlation between the factor which determines the polarity of these forms and the activity of A.T.P.-ases. Carbon dioxide, as we pointed out, represses the formation of the zooid at one end. Of course, there are a number of interpretations as to what carbon dioxide does. It is interesting that the pH optimum of all A.T.P.-ases is well on the alkaline side. The pH of the cell is not known, but carbon dioxide-saturated sea water is about 5.0. Carbon dioxide does penetrate cells, as we know from permeability studies, and, by making the cell acid, may lower the A.T.P.-ase activity.

In summarizing these results, we can say that A.T.P. is present in the frog egg, and that it breaks down when energy is needed, as in the case of anaerobic conditions. We also know that as development proceeds in the frog egg, enzymes which split A.T.P. do appear in greater and greater concentration. And finally, from this last work we can say that during the process of morphological dedifferentiation the A.T.P.-ase activity disappears and reappears when differentiation takes place once more. These few facts suggest that A.T.P. is an energy source for differentiation.

DIVISION OF MYCOLOGY

A NEW METHOD FOR THE EVALUATION OF ANTIFUNGICIDAL CHEMICALS

By SAMUEL M. PECK*

New fungicides are evaluated according to certain standard procedures. There are excellent *in vitro* techniques which give data concerning the necessary concentrations and the rapidity by which the fungicides kill various pathogenic organisms. Thorough toxicity studies are necessary before clinical trial is attempted.

While clinical evaluation is time-consuming and expensive, no *in vitro* method can take its place. Too often, however, the clinician is confronted by discrepancies between the laboratory data and clinical effectiveness.

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* Dermatologic service of the Mount Sinai Hospital, New York. This paper was prepared with the assistance of Rose Bergamini and presented at the meeting of the Division on January 28, 1949.

A useful fungicide must not only have a high index of clinical cure, but there must be a minimum of irritation from its use. This irritation is either primary or due to sensitization. When the irritation is primary, because of the necessary effective concentration, the fungicide cannot be used in contact with living tissue. Such a factor is quickly evaluated. The irritation due to sensitization is of two types. When sensitization is due to an idiosyncrasy of the fungicide itself, we have well established procedures which give us reliable information concerning the sensitizing index of any chemical.[1] The second type is based on a different mechanism which will be described.

In most clinical applications of fungicides, we are dealing with infections of the skin or hair. Because of this, the evaluation of any fungicide must take into consideration the special and specific conditions which are present in most of these fungus diseases. The majority of the fungus infections which we are called upon to treat are those due to the dermatophytes; and of these, dermatophytosis of the feet (athlete's foot) is the most common.

Enough statistical data[2] are available to show that more than 75 per cent of the population, depending on the age group and the season of the year, will show active clinical evidence of dermatophytosis of the feet. Of these, more than 50 per cent will develop various degrees of sensitization to fungi and or their products. In many instances this sensitivity is accompanied by allergic manifestations which are known as dermatophytids.

It is the common experience of the clinician that, even with fungicides which are not primary irritants and, apparently, have a low index of sensitization, irritations are often encountered. These symptoms are due to the second type of allergic reactions, which are seen during the course of treatment with fungicides and are not due to the sensitization to the fungicides themselves. Such allergic phenomena are due in most instances to active precipitation of allergic reactions of the dermatophytid type.

The explanation for their occurrence has been that, just as in the Herxheimer reaction in syphilis, the fungicide causes a rapid destruction of the fungi which in turn causes a sudden release of toxins or antigen. In the sensitized individual, the "id" reaction results. Apparently no one has suggested the possibility that the fungicide itself may actually stimulate the formation of antigens by the fungi.

An ideal fungicide, then, would be one which is not primarily irritating, which has a low sensitization index per se, which suppresses the growth of the fungi, and which, at the interphase between fungicidal and fungistatic activity, does not stimulate the pathogenic organisms to produce antigens.

We believe that, in the method which we are about to describe, the last factor can be evaluated.

Materials and Methods

Our method is based on the study of the effects of addition of fungicides on the production of trichophytin. Trichophytin is the watersoluble antigen found in the pellicle and the filtrate of a culture of fungi on Sabouraud's bouillon. It has been shown by many investigators that the positive skin reactions following the intradermal injection of trichophytin is due to a specific sensitivity resulting from a fungus infection. Trichophytin has been identified with the specific sensitizing factors which are elaborated by the fungi in the course of the infection and are responsible for allergic reactions or dermatophytids.[3]

The usual procedure for the preparation of trichophytin consists of inoculation of Sabouraud's bouillon with trichophyton gypseum or trichophyton interdigitale contained in a large Erlenmeyer or Roux flask at a pH of about 6.5 to 6.7. Growth is allowed to proceed at room temperature for ten to twelve weeks until a large surface pellicle is formed. The pellicle is then triturated with sand in the mortar after preliminary freezing with solid carbon dioxide. A sufficient amount of the bouillon is added to make a slush. The semiliquid mass is placed in the shaking machine for 24 hours. It is then kept in an incubator for another 24 hours and again shaken for 24 hours. The mass of growth in broth is then filtered through several thicknesses of filter paper and the filtrate is passed through a Seitz filter. The final filtrate has to be diluted before use in order to avoid non-specific reactions. This is the usual commercial trichophytin as it is now available for diagnostic and therapeutic purpose.

A modification of the routine method for the production of trichophytin is used in our laboratory.[5]

It has been shown in previous communications that T. gypseum was capable of producing a more alkaline pH in Sabouraud's bouillon when grown on a medium with an initial pH of 4.0 to 8.0. At an initial pH of 9.0 to 10.0 the organism acidified the medium. We could show that there was a relationship between the changes in the hydrogen ion concentration of Sabouraud's bouillon and the appearance and the concentration of the skin test factors (trichophytin). The skin test principle was apparently formed in minimum quantities or not at all as long as the pH kept dropping, but the concentration of this factor roughly paralleled the rise in the hydrogen ion concentration. Once this rise in pH takes place, enough trichophytin is formed in the bouillon to compare favorably with the long drawn out procedure which is routinely used. A rise of pH of only 2 on the acid side will give enough trichophytin in as short a period of time as ten days.

Sabouraud's bouillon, used in our experiments, was prepared by adding 40 grams of dextrose and 10 grams of neopeptone (Difco) to 1 liter of distilled water. It was then placed into an Arnold sterilizer for ten minutes and finally filtered through Watman's No. 4 filter paper while the solution was still hot. One to 10,000 merthiolate was added as a preservative.

The Sabouraud's bouillon was inoculated with T. gypseum; the same strain of organism was used throughout the experiments. A ten days' culture of T. gypseum on a slant was used. A few cc. of saline were added, shaken vigorously, and 1 cc. of the resultant spore suspension was used for inoculation into each flask. One hundred cc. of Sabouraud's bouillon was usually used in each flask. In some of our experiments, only 20 cc. were used. There were a number of control flasks for each experiment.

pH readings were taken at frequent intervals to determine the changes in the hydrogen ion concentration. A Beckman pH meter using glass and calomel electrodes (mild mercurous chloride) was used for the pH determinations. The flasks used to grow the organisms were constructed with a side neck at the base. This allowed frequent milkings with a minimum danger of contamination, and samples were taken equally at intervals of 7, 14, and 21 days.

The fractions were tested for their trichophytin content by the usual intradermal trichophytin technique. In this test, a dilution of trichophytin is used which is known not to give non-specific reaction. It is usually necessary to dilute the bouillon as we have prepared it in our experiments 1–20 or 1–30, using a standard trichophytin made in our laboratory of known potency as a control. One-tenth cc. are injected intradermally. The reaction is read in from 48 hours and sometimes up to five days for delayed reactions. A positive reaction is characterized by an area of redness and induration 1 centimeter or larger in diameter. In well defined reactions, papules and vesicles may be in the area of redness. Reactions several centimeters in diameter are not unusual. Sometimes, the reaction does not appear for four to five days and is known as a delayed reaction. The reaction may last for a week or longer and gradually disappear with scaling. It is analogous in many respects to a tuberculin test. It is usual to grade the reactions from 1 to 4 plus. The same concentration and amount of Sabouraud's bouillon is injected at the same time to determine the rare sensitivity to Sabouraud's bouillon itself.

Six to twelve patients were used in each series of tests for each fraction, so that negative as well as positive reactions were present in each series. In this way, non-specific reactions as well as specific reactions could be evaluated. The concentration of trichophytin in any one flask was determined by the comparative reactions in sensitive patients to equivalent concentration of the filtrate from bouillon cultures containing the fungicides as compared to the filtrate from control flasks.

Experimental Part

In the following series of experiments, we will attempt to give typical examples of our experiences of the evaluation by our method of some of the old and well known fungicides as well as some of the newer ones.

Iodine:

To each of three flasks containing 20 cc. of Sabouraud's Dextrose bouillon there was added 1 cc., 0.1 cc.. and 0.01 cc. of a 2 per cent alcoholic iodine solution. Each flask was then inoculated with 1 cc. of spore suspension of a ten-day-old culture of T. Gypseum.

Results:

Iodine is very little soluble in water. There was a definite effect on the growth of the organisms by the iodine in each of the flasks as compared with the control. At the end of fourteen days, there was a definite suppression of growth in all of the flasks containing the iodine. In spite of this the concentration of trichophytin found in any of the flasks at that time was greater than that found in the control flasks which had a more abundant subsurface and surface growth.

Sodium Benzoate:

To three flasks, each containing 20 cc. of Sabouraud's bouillon there was added 1 cc., 0.1 cc., and 0.01 cc. respectively, of a 30 per cent aqueous solution of sodium benzoate. One cc. of the same spore suspension as above was added to each of the flasks.

The addition of 1 cc. of 30 per cent aqueous solution of sodium benzoate inhibited growth. There was some retardation of growth in the flask containing 0.1 cc. and the growth did not differ from that in the control in the third flask.

The pH changes in each of the flasks paralleled the degree of growth. As was to be expected, there was no trichophytin formed in the flask in which there was no growth. In the second and third flasks, practically the same amount of trichophytin as in the control was found.

Sodium Undecylinate:

To each flask containing 100 cc. of Sabouraud's bouillon, there was added 0.05, 0.23, 0.5, 1 mg., 2 mg., and 5 mg. of sodium undecylinate, respectively. Each flask was inoculated with the spore suspension as previously.

There was no growth of fungi when 2 mgs., or more, of sodium undecylinate were added. When no growth was visible, there was no change in the pH of the bouillon and no trichophytin could be demonstrated. In the flask which contained 1 mg. of sodium undecylinate, there was scant subsurface growth at the end of 21 days. The pH changes were slow in developing and there was a moderate rise in the hydrogen ion concentration beginning with about the 14th day.

The addition of 0.5 mg. sodium undecylinate to 100 cc. of Sabouraud's bouillon had a fungistatic effect. Subsurface growth was evident on the 14th day. This increased in amount slowly, but at the end of 21 days there were still no surface pellicle. A moderate amount of trichophytin, less than that found in the control flasks, could be demonstrated on the 14th and 21st days.

The addition of 0.2 mg. of sodium undecylinate still exerted an evident fungistatic effect, but not so much as when 0.5 mg. were added. However, there was as much trichophytin formed in this flask as there was in the control flasks.

Here, we have a fairly good fungicide. The concentration of trichophytin depends on the degree of growth of the fungi. However, with lower concentrations of sodium undecylinate, the concentration of trichophytin formed was greater than was to be expected from the degree of growth seen.

Sodium Propionate and Sodium Caprylate:

To 100 cc. of Sabouraud's bouillon, a mixture of 5 mg. of sodium propionate and 5 mg. of sodium caprylate was added. There was a definite retardation of growth as compared to the control. The pH changes were reflected in the degree of growth. The trichophytin formation was apparently less than in the control flasks for the same period of time. Here, as compared to the sodium undecylinate, there was apparently even less trichophytin formed than in the same comparative amount of growth. The formation of this complex antigen seemed to be depressed.

Boric Acid:

To 20 cc. of Sabouraud's bouillon in each of three flasks, there was added 1 cc., 0.1 cc., and 0.01 cc., respectively, of a 1 per cent aqueous solution of boric acid. While 1 cc. of boric acid showed a marked retardation of growth, at the end of 14 days there was more trichophytin formed in that flask than in the control flasks which showed much more growth. When 0.1 cc. of boric acid was added there was moderate retardation of growth at the end of 14 days and the trichophytin formed was still more than the control. When 0.01 cc. was added to the flask, there was apparently a stimulus to growth. The trichophytin concentration was increased as compared to the control.

Boric acid, evidently, according to our method of evaluation, is a poor fungicide.

Amorphous Penicillin G:

To 100 cc. of Sabouraud's bouillon in each of two flasks, 300,000 units and 100,000 units, respectively, of amorphous penicillin G, were added. Spore suspension was added in the usual way. Growth was retarded in both flasks until the 21st day. At that time, subsurface growth became evident in the flask containing the 300,000 units. The trichophytin formation was minimal. There was some subsurface growth at the end of 21 days in the flask containing the 100,000 units—more than in the flask containing the 300,000 units. The pH curve began to rise about the 21st day, but no trichophytin was formed.

Crude Penicillin:

In this experiment, crude calcium penicillin, obtained through the kindness of Lederle, containing 310 units per mg. was used. To flasks, each containing 100 cc. of Sabouraud's bouillon and inoculated with the spore suspension in the usual way, there was added 1, 3, and 5 grams, respectively, of this crude penicillin. To two other flasks, there was added 3 and 5 grams of this penicillin, respectively, but prior to its being used, the penicillin was heated in a water bath for three hours at 57 degrees centigrade to destroy any penicillin activity.

In the flask containing the 1 gram of unheated penicillin, scant subsurface growth was evident at the end of 23 days. There was no rise in pH. There was a marked reduction in formation of trichophytin. In the flask containing 5 grams of unheated penicillin, there was no evidence of growth, no change in the pH curve, and there was no trichophytin formed.

In the flask containing 3 grams of the heated penicillin, there was scant subsurface growth at the end of 23 days; the pH curve was almost flat, and the amount of trichophytin was markedly reduced.

With 5 grams of heated penicillin, there was no growth, no change in pH curve, and no trichophytin could be demonstrated.

Discussion

While some interesting conclusions can be drawn concerning the experiments just cited, a great deal more work will have to be done to determine the importance of this type of evaluation of a fungicide.

There is no doubt that the determination of the changes in the hydrogen ion concentration in a culture of fungi on Sabouraud's bouillon is a very good gauge for the formation of trichophytin. The modified method for the manufacture of trichophytin suggested is time saving and apparently just as efficacious as the older methods now in use.

Iodine has been known to cause irritation in patients with fungus disease, and, in most instances, this has not been due either to primary irritation or to sensitization to iodine itself.

Experiment 1 offers a good explanation for such clinical observations. It is evident that as iodine suppresses the growth of fungi it apparently effects the metabolism of the fungus in such a manner that trichophytin formation is enhanced.

Boric acid has been an old standby as a safe treatment for the fungus infection which is accompanied by allergic manifestations. Experiment 2 indicates that it is a poor fungicide, since in low dilution it may apparently enhance the growth of the fungi and, what is more important, actually stimulates the formation of trichophytin.

Sodium benzoate and sodium undecylinate are good fungicides since the concentration of trichophytin drops as the fungus is retarded in growth.

Sodium propionate seems to bear out its clinical evaluation; namely, that it can be safely used even in highly sensitive individuals. It can be seen that the formation of trichophytin is diminished not only because the growth of fungi is retarded, but because of the effect of this chemical on the metabolism of the fungus. From this viewpoint, it is more valuable than sodium undecylinate.

In previous communications[6] it has been shown that amorphous penicillin G contains a factor or factors which retard the growth of fungi. It was shown that these factors were not analogous to penicillin G. When the amorphous penicillin is refined, these products are apparently removed. In the experiment cited here, it was further shown that apparently these factors are contained in crude penicillin and that they are not affected by heating, which further differentiates them from penicillin itself.

It is of special interest to note that these fungistatic and fungicidal agents in crude penicillin, as they retard the growth of the fungi, markedly reduce the formation of trichophytin. Apparently, we have there a very valuable agent against fungi if it can be isolated and concentrated enough to be used clinically.

Summary

A method is presented for the evaluation of fungicides which attempts to demonstrate, not only their fungicidal and fungistatic activity, but their effects on the metabolism of the fungi as far as the formation of trichophytin is concerned. This is of value since trichophytin is identified with the antigens elaborated in the course of fungus disease which are responsible for the allergic manifestations often associated with them.

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SECTION OF PSYCHOLOGY

THE ANNUAL SCIENCE TALENT SEARCH FOR THE WESTINGHOUSE SCHOLARSHIPS.[*]

By H. A. EDGERTON[†] AND S. H. BRITT[‡]

The eighth annual Science Talent Search has been completed and the forty winners will soon be announced. Back of this is one of the most interesting programs in which we have ever participated.

In the spring of 1942, the Annual Science Talent Search was inaugurated by Science Service, and organized as an activity of Science Clubs of America. It has been conducted annually with the administration of Science Service. The entire program, including awards to the winners, was made financially possible by the Westinghouse Electric Corporation as a contribution to the advancement of science in this country.

Its purposes are: (1) To aid in the discovery and development of scientific aptitudes of boys and girls in our high schools. (2) To aid in creating more interest and activity in science among the youth of the country. (3) To emphasize to the public the necessity of developing that facet of our human resources—scientific talent.

We wish to give a brief summary of the techniques used, some notion of how they operate, and a look at what happened to the contestants in later years.

When we were asked one morning, "How should one make such a selection as called for by the general plans of a Science Talent Search among high school seniors?" we had to do more than note that "that is a good question."

We had to find answers to these questions: (1) How does one recognize persons, now seniors in high school, who later will be creative scientists? (2) What selection techniques, adequate to the problem, can be administered satisfactorily in high schools? (3) Could the procedures be made fairly objective and such that their cost would not be prohibitive in time or labor? Then came that $64 question, to which we had to give an answer:—"What kind of person should we seek?" What was our best bet regarding the identity or description of a creative scientist when a senior in high school?

Undoubtedly, such persons should be "bright" and able to do academic work of high quality. They should have some background in science, and should have shown by their records, both in and out of school, a strong interest in science. In addition, they should have exhibited such traits as social competence, initiative, resourcefulness, and good work habits. This picture of the potential scientist suggests a person who is intellectually capable, interested in science, and a leader among his fellows. Such criteria for selection very likely have resulted in overlooking some persons who later will be leaders in science and invention, but it is believed that from an *a priori* probability point of view, they have justified themselves.

The competion is really two contests—one for boys and one for girls. It has been agreed each year that the ratio of boys to girls throughout the competition would remain constant and, also, approximately equal to the ratio of boys to girls who entered the contest.

In order to carry out a selection procedure which would have validity and also be administratively economical, the "successive hurdles" technique has been used every year. This means that all contestants must pass the first hurdle, but some will survive this hurdle and some will not; the survivors of the first hurdle then face the second hurdle; the survivors of the second hurdle go on to the third hurdle, etc., until there remain only the scholarship winners. Such a method has its maximum validity when the successive hurdles are applied in decreasing order of validity.

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[*] This paper was presented at the meeting of the Section on January 17, 1949.

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Accordingly, the successive hurdles were set up as follows:

1. *Science Aptitude Examination.* This was to select those who have the aptitude to study science in colleges and universities, but without too heavy a premium on knowledge of science.
2. *High School Record.* To select those who have demonstrated their ability to profit from formal academic training.
3. Score on *recommendations* furnished by high-school teachers, covering such categories as scientific attitude, resourcefulness, initiative, work habits, social competence, and mechanical ability.
4. Rating on the essay submitted on the topic, "My Scientific Project."

The first three hurdles reduce the numbers of contestants to 300. The fourth, plus all the accumulated evidence, is the basis for selecting the winners—40 boys and girls who attend the Science Talent Institute in Washington, D. C., each year, hear and talk with outstanding scientists, and compete for the top scholarships. The remaining 260 of the 300 are honorable mentions, and they well deserve that honor. They are within the top 20 or 25 per cent on the examination, in the top 10 or 15 per cent of their high-school class, and have shown great interest in science, good work habits, social leadership, etc., in terms of what they have done.

Including the eighth Science Talent Search, there have been for all eight years 320 Winners, and 2,080 Honorable Mentions.

The top scholarships—2 each year—are awarded on the basis of the judgment of the contest judges. Their information includes all that available in selecting the winners, *plus* the information and opinions gained through standardized psychological interviews with the winners. Each winner is individually interviewed by each judge.

Then comes the question regarding the empirical validity of the selection procedures. Do those who were winners succeed in greater measure as scientists than those who were awarded honorable mention; and did the honorable mentions succeed in greater measure than those who won no honors at all? Then there is the additional question, "Did the identification and public acclaim produce the difference in achievement, if any, between those who were honored and those who were not?"

Each year a follow-up questionnaire is mailed to all who won honors in each of the searches, and to all contestants of the first and second searches.

A study of the questionnaires of the boys in the first Science Talent Search was reported last summer in the *American Scientist.** This study, based on the first three questionnaires, shows the following:

1. More of the honors group (called the A group) majored in Science than did the non-honors (called the B group) contestants.
2. More of the A group went to college than did the B group. Despite war conditions, each year the winners had the largest per cent in college, the honorable mentions the next largest per cent, and the others the least in college.
3. In terms of college grades, based only on those attending college (and with the problems of approximately equating marks from many colleges) the winners averaged 3.55 the second year and 3.52 the third year, (A = 4, B = 3, etc.); the honorable mentions 3.48 the second year and 3.42 the third; while the other group averaged 3.00 and 2.99. The entire group certainly has set an outstanding record. Similar data have been shown for the girls.
4. Membership in honorary societies might be a clue to outstanding achievement. At the third year, 49 per cent of the winners had become members of an honorary society, 17

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* Edgerton, Harold A., Britt, Steuart Henderson, and Norman, Ralph D. "Later Achievements of Male Contestants in the First Annual Science Talent Search," *American Scientist.* 36: 403–414, 1948.

per cent of the honorable mentions, and of the other, 5 per cent had become members.

5. In terms of scholarships and fellowships, other than the S.T.S. awards, the A group has been considerably ahead of the B group.
6. Considering only the contestants' first choice of professional field, striking results are found. Considering the scientific field broadly, 73 per cent of the A group indicated some scientific area as their first choice. This includes 87 per cent of the winners. This may be compared to 55 per cent of the B group giving a similar indication.
7. The question, "What are the main things you have learned about science during the past year?" revealed that, while the differences in the responses of the A and B groups were not great, this question would become more valuable as time passes by.
8. The item, "List and briefly describe any *special* scientific or professional apparatus or other mechanical devices which you have become competent to use since January of last year," yielded no basis for distinguishing between the two groups. It is possible, though, that had the military aspects of the situation not come so strongly into the picture, involving as it did introduction of our contestants to a tremendous variety of technical and scientific apparauses, more differences would have been found between our groups. This item in the questionnaire may ultimately become more useful in the later years if, as we believe, the A group demonstrates superiority.
9. The items, "List your principal contributions in the form of publications, inventions, patents, etc.," and, "Describe any research or other special work which you now have in progress," produced no answers which distinguished between the two groups, A and B.
10. In the first year, 5 per cent of the A group and 6 per cent of the B group reported membership in one or more professional societies. In the second year 11 and 10 per cent respectively reported, whereas in the third year 17 and 13 per cent mentioned such membership. The A group thus has a slight but insignificant excess in the third year. The trend is about the same for the Winners.
11. In the follow-up study of the male contestants in the Second Annual Science Talent Search, some evidence was offered to show that selection rather than public recognition was associated with such evidences of achievement as we have just listed. Using a sample containing only the B group—no winners nor honorable mentions—there was some relationship of the selectors (test score, high-school record, and recommendations) to successes.

Using data from only the first three years of follow-up of the contestants in the First Annual Science Talent Search, we believe that boys selected by the successive-hurdles mentioned as superior in scientific aptitude tend actually to be superior in terms of several objective criteria. Of the ten criteria touched upon, six have been shown to be distinctive:—majoring in science; extent of education; college grades; honorary societies; scholarships or fellowships; and choice of professional field. The other four criteria yield no statistical significance between the ranking and non-ranking contestants, except possibly "things learned about science," which may be significant in two out of three years. However, war service should not be overlooked as an important factor with respect to the question on the use of scientific apparatus and in other criteria.

The follow-up study is continuing,—questionnaires each year. A few years from now we hope to be able to summarize the results in terms of ten years of growth of science talent.

SECTION OF ANTHROPOLOGY

SOME PRELIMINARY CONSIDERATIONS OF LARGER UNITS IN CHINESE SOCIAL ORGANIZATION

By MORTON H. FRIED[*]

The literature of anthropology is notoriously weak on China. This lack is being corrected at a rapid rate with such additions as the work of Fei Hsiao Tung, Chang Chih-i Lin Yueh-hua, Martin Yang, and Francis Hsu, all of whom have contributed their volumes within the past ten years. However, there is no adequate description or analysis of larger and more intricate community organizations such as larger market towns or county seats. It will be the purpose of this paper to give some general descriptive facts concerning the county seat as a discrete unit of Chinese society and from that base make some analyses of the interrelationships between such type-units and the country-side in which they appear.

The author was fortunate enough to have resided in a Chinese county seat for a period of more than a year, being supported in this field study by the Social Science Research Council. The facts given in this paper are drawn mostly from data collected in the town of residence.

Seen in a romanized form, *Ch'u Hsien* is a common Chinese place name. In the original characters there is no such confusion, the *Ch'u* character is a phonetic equivalent that has no meaning other than to designate this place. The *hsien* character is of general usage, it stands for 'county' and when combined with a place name it may mean either a county or a county seat. Thus, Ch'u Hsien means either the whole county that lies just inside the Anhwei provincial border across the Yangtze River from the national capitol at Nanking, or the seat of government of that county, an ancient walled town.

Ch'u Hsien is in the Yangtze Plain and raises rice, winter wheat, corn, cotton, and sesame as major crops, and soy beans, barley, yellow beans, kaoliang, buckwheat, and hemp among the secondary crops. There are two crop yields a year: the fall harvest being mainly of rice and corn, the spring harvest of wheat and beans. The gardens, which are quite separate from the fields, produce twelve months out of the year. However, the winter yields are small and the people of the town, as well as their rural neighbors, put out a supply of vegetables to dry. After salting, these foods are reserved for periods of winter scarcity. Pigs, chickens, ducks, and geese are kept in abundance within the town and dogs are common. No water buffalo are present except those which are being kept for market, there being no use for such animals in the town. Burros, while frequent in the countryside, are used in the town only for turning grindstones and they appear in the shops of oil producers or the millers of corn.

The landscape of the town is, of course, quite different from that of the countryside. Ch'u, in particular, is a most beautiful spot with its aged walls and three hoary pagodas. But in addition to the normal difference in contour due to a variation in building types, there is a notable change in the appearance of the land. As one approaches a town or city he can see the paddy fields give way, quite suddenly, to the small strip gardens that characterize Chinese horticulture. The systems of cultivating gardens differ markedly from those used in the fields. These differences are reflected in many ways: linguistically, in the use of a separate specialized vocabulary for each; in social structure, by the existence of a separate guild for each; the contrasts between these two organizations; and by the contrasts in division of labor within the respective families of a farm and a garden. There are many additional points of dissimilarity, tools, standard of living, land ownership, and daily routine, but such a discussion lies outside the present paper.

The town we are using as a model is itself an ancient and walled citadel with a long military and political history. The walls enclose the town in a roughly circular shape with gates at the four cardinal points. Only one of the four gates has its tower and one other gate still has remnants of its outer wall, a feature that shows up clearly on the old maps of Ch'u, which may be found in the "Ch'u Chou Tih Chih" or local gazetteer. The town is not oriented about a single point or even about one main street. The approximation of a main street, Central Street, is only one of several important streets. It is, however, the center of one of the two major commercial areas in Ch'u.

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Ch'u is a town of 31,000 people, most of this population being concentrated within the walls, with many families living outside but within about one-third of a mile of the walls. These people are concentrated into four centers, one at each gate, the group at East Gate being the largest and most significant. This is the gate which contacts the Tsing-puh Railroad whose first express stop on the way from Pukow is Ch'u Hsien. The street which runs from Central Street to the railroad is called Great Eastern Street and it has two sections, the parts within and without the walls. Along Great Eastern Street are numerous grain shops, large stores, schools, restaurants, in short, all of the services that may be found on Central Street save the government banks, which prefer to locate themselves behind the massive walls. The importance of Great Eastern Street is enhanced by the proximity to the Ch'u or Sandy River, a small stream up which, six months or more of the year, small boats come with goods. The East Gate Outside is also the site of the city's one mechanized industry, a wheat flour mill which operates by steam engine. It should be said that there are no notable conflicts between the East Gate Outside and the town proper. To a large extent, the important merchants of the town proper have establishments also at East Gate; certainly the Chamber of Commerce and the Guilds crosscut the geographical difference.

Not far from Central street are two daily produce markets. East Gate Outside has one large and one small market and South and West Gates have small markets. On south Central Street, there is another produce market, making the total three large and four small daily markets for the town. Each day, starting at about seven o'clock in the morning, the gardeners, fishermen, and small merchants assemble with the items they have for sale. The markets are not the only places at which food may be bought. Indeed, the daily produce markets include sellers of grain only at such times when great price rises place the grains outside the reach of groups normally used to consume them, at which time they must be purchased in minimum quantities. On the way to market many vendors cry their wares, hoping in this way to sell as much as they can as early as possible. Most of the people of the market leave as soon as they have made their purchases or sold their goods. The markets in Ch'u Hsien are not areas of great social contact. For the most part social activities here are confined to the process of exchange. We should modify this in part, however, since there is some, though not extensive, social contact among the various merchants and peddlers in the market. Particular places are associated with certain individuals and there is some tendency for certain products to occupy a definite area. There are many hawkers who prefer to make the bulk of their sales outside the market areas in order to avoid too keen competition of price and quality. The factors that draw a seller to the market are the furnishing of a point of concentration of customers and, much less important, the presence of pleasant social contacts in the periods between sales. For the buyer, the market is an area where he may see the condition of the market and enjoy lower prices through direct competition. The market is also a feature of social control over food retailing, a reflection, on a smaller scale, of licensing restrictions on larger merchants. This type of factor was most apparent during the periods when the Nationalist Government tried to counteract inflation by enforcing price controls. During one such drive, the local county government was given the responsibility of enforcing the price ceilings, and to accomplish this, the local police were instructed to herd the peddlers into the market areas. Since the markets are simply transient collections of sellers at certain points, the government felt that control would be more easily enforced if the market, particularly the largest market, had a more formal layout. Steps were taken to make a permanent market area out of some vacant land lying between the market street and the Temple of Confucius, land belonging to the hsien. The police were to enforce the move into this area by confiscating the scales of peddlers found making sales outside of this area or other market sectors. The program was carried out after a fashion, but the reaction of the gardeners was almost unanimous. With the exception of the fishermen, who are a depressed income group and require daily sales to insure their subsistence, the market was almost empty of items for sale. When the gardeners' guild was queried, the reply was that the produce was as yet unripe and therefore unfit for sale. The situation became quite desperate when the action of the gardeners was paralleled by similar refusal to trade by the merchants. The situation was partially resolved with the formation of a black market in commodities, but the cashless middle income group suffered badly.

The separate groups resulting from the extended division of labor in Ch'u are numerous. The most significant groupings include merchants, who range from the great 'laobann,' controlling several shops and with influence in different kinds of businesses, to the 'hsiao paw,' the small sidewalk merchant whose entire stock may not be worth more than two or three U. S. dollars. There are also domestic servants, a large group made of individuals of both sexes, who hire out by the season and who, for the most part, are recruited from farms or from poor laboring families with which the employer has some direct or indirect contact. The artisans are quite numerous and include blacksmiths, silversmiths, weavers, basket-makers, bean-curd makers, tailors, and a host of others. The services are conspicuous, probably most important being the men engaged in transportation, the mode being wheelbarrow, back, ricksha or cart. There is also a large category of unskilled, unallied labor. These males furnish a reserve for agricultural and horticultural work in season as well as daily labor on transitory jobs. Such men may, for example, be employed to pull up the weight on a pile driver or to carry bricks and tile in the construction of a building. Lastly, there is the major group of gardeners.

The absentee landlords in Ch'u hardly deserve to be categorized separately. Despite the legitimate existence of such a specialization, the number of people who derive no income and fill no function other than that of landlord is small. Still smaller is the number of families whose income or function is circumscribed by the ownership of land, since, of the small number of landlords who are completely occupied in this work, the greatest part is engaged on behalf of a family or some social group, the other adult members of which are occupied in other activities. These activities are largely professional or commercial, with official position high on the list of professions. The corollary of this situation, that the political bureaucracy is largely made up of members of landlord families, has had great effect on the history of China and its implications in the social organization of the town and particularly of the county seat are profound. The correlation of office and land ownership is highest in the upper brackets of government service. On the hsien level it includes magistrates, secretaries, bureau (k'ou) heads, and the leaders of county districts. The correlation falls sharply with lower clerks, heads of population units (bao and chia), and vanishes before the level of policemen, servant, or attendant is reached.

There is a tendency for the specialized occupational groups to polarize in two directions independent of the job specialty, this polarization being roughly along the lines of income, more particularly, along lines which combine in a subtle way, income, relation to the system of production, and the historical social position of the individual. Here we meet again the fact of the horizontal nature of absentee landowning which includes the specialties of higher official, prosperous merchant, and prosperous artisan. In the last two specialties, the individual ceases direct work at the specialization becoming, at first, managerial and later even removed from the enterprise. Furthermore, the leadership of the various guilds, the formal structures based on the division of labor, is always vested in the 'gentlemen' of the town, a role that is the very essence of absentee landholding. The leadership of the occupational specializations is thus always vested in a class rather than in the specialties themselves.

The lower elements within a trade also converge outside of their specialties, forming a reciprocation of the upper class phenomenon. These convergences, however, are subject to the scrutiny and censorship of the upper class and there the process is completely unilateral and reciprocation ends. The groups of lower class origin, then, are controlled and patterned by the upper class, if they persist they operate outside the law and must do so in secrecy. An example from Ch'u: as a reaction to the steady increase in rents, there was a local movement among the petty bureaucrats, teachers, and small merchants to form a tenants' union. The association was formed and it petitioned strongly, addressing itself to the public to gain members but appealing to the magistrate for corrective measures. The magistrate advised the union to disband and present petitions individually to the respective landlords. The advice was quickly followed by an order, and, fearing punitive action, the association dissolved.

It is incorrect to think of the county seat or town as other than an integral part of the county, but it is also an error to hold that towns are merely large villages. The basic wealth of Ch'u is in the production of the peasants who constitute a majority of the population of the county. To this agricultural production may be added the labor of artisans who fashion usable commodities from raw materials. The bulk of the town population, however, relies on the exploitation of these elements for its success. For example, the two most immediate drains on the incomes of tenant farmers are rents owed to the landlords and interest owed to usurers, as well as taxes and assessments owed the government. In Ch'u there are two systems for assessing rent. There is a flat and standard amount paid semi-annually, so many 'tan' of rice and so many of wheat, or a percentage arrangement, 40 per cent of the staple crops in season being the usual figure in the area. Thus, at the outset, the peasant parts with the major part of the return from the land he has cultivated, giving the greatest part to his landlord. The merchant, dealing in most cases with landlords of farmers is also dependent on the agricultural production of the hsien though the forces which condition the farmers' lives are mediated in his.

The case for the independence and uniqueness of the town as a social phenomenon depends on a recognition and analysis of the nature of the bureaucracy of government, the structure and importance of military units which are associated with the towns, the concentration of capital in a restricted area, and the concomitant presence of a class of wealthy families and individuals. These factors which are in part inevitably associated with the increase in the size of the community are, nonetheless, discrete and unique phenomena which do not appear on lower organizational levels and which carry in their wake altered conditions which reshape the character of succeeding developments.

Guild phenomena are so negligible in village organization in China that no monograph contains any discussion of them. It is only on the town level, where there is a concentration of specialists that the guild fills a needed role. The farmers' guild, the one such organization that is extended to rural villages in Ch'u, is not the spontaneous creation of farmers but is a calculated structure imposed from without, whose officers are semi-agents of the state.

Another example of the uniqueness of the town as a social form may be found in the institution of apprenticeship, a development which, in China, is largely independent of the guilds. In the villages, learning is by the imitation and observation of parents whether they are engaged in agriculture or in subsidiary handicrafts.* In the town, a whole new arrangement is found whereby young males are instructed in the techniques of some handicraft or commercial specialty, a relationship which includes a new social relationship that has no proper counterpart in the village.

If a town is related to its agricultural hinterland, it is also vitally connected to other towns and cities and through them with more remote entrepôts and the world community. Some of these connections are clearly revealed in a consideration of the method by which currency enters the town economy, an analysis that points up, once again, the ties of town to country. Beginning with the production of grain, we have seen that a large portion is immediately earmarked for the landlord, an individual who frequently, and in Ch'u Hsien generally, lives in town. The landlord, after he has separated his rents into amounts he will have to pay out as taxes and the part he expects his family to consume, will sell his surplus in order to have money to meet other expenses such as other items of diet, education, clothing, and perhaps rent on his own house. The last, however, is generally paid in rice if the tenant is known to be a landlord.

The grain that is sold by the landlord will go to a local grain shop or to an outport grain store; in either case the rice is destined for the outside. The payment for the exported rice is always made in currency, just as commodities which are purchases from the outside are bought with cash and not bartered for.

Another way in which currency enters Ch'u is in payment of the services of government employees and officials, categories which range from the Magistrate to the railroad laborer. In Ch'u it is clear that the income the town derives from the government is far short of the amount it sends out of the community in taxes. Another factor is that, while the greatest part of the tax payment is in grain, the bulk of salary payments is made in currency.

Money as a commodity leaves Ch'u as well as entering it. A certain amount is drained off institutionally in the form of taxes, the merchants paying their taxes in currency as opposed to landlords who pay in grain. Most of the outgoing cash, however, is expended by merchants who buy consumer goods which are largely the product of Shanghai, Wusih, or the manufacturing cities of foreign countries. The flow of goods imported from the outside should not be underrated, since it is through such medium that much enculturation and culture change is proceeding. The store system in Ch'u has special shops carrying outport items. One type of store, the 'yang-dean' sells items associated with the western countries, such as matches ('yang-huo') and kerosene ('yang-yu'). Other shops, called 'kuang huo dean,' sell items that originate in the two Kwang provinces of the Southeast.

We have treated very briefly some aspects of the nature of town phenomena in China, giving special attention to the relationships between the town and the countryside and the town and the larger cities. The examples have been based on data collected in Ch'u Hsien. Generalization from facts gathered in one place is a procedure fraught with danger. Sophisticated Chinese informants warned continually during the study in China that the information did not apply to the country as a whole. Yet, there are many factors which we have touched upon that appear throughout China, and the regularity is more pronounced in the more restricted area of the Yangtze Plain.

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* This does not refer to literary training, which, in the village as in the town, is delegated to a specialist.

It has been shown that a town or county seat, even one with so many outgoing relationships as Ch'u, is organically and functionally tied to the agricultural hinterland. The distinctiveness of the town as a unit of organization has been outlined and the way paved for a later and more important comparison and contrast of county seat and town in some future paper. Lastly, it has been indicated that the town is economically poised between the agricultural hinterland and the large cities and entrepôts which are contacted by railroad, stream, or motor road. Though the present paper has been most concerned with tracing certain economic relationships, all three of the points made above can be extended with materials drawn from the realms of religion, political structure, and social organization.

THE NEGRO KINGDOMS OF THE SUDAN

By JOSEPH GREENBERG*

Monarchical patterns are widespread in Negro Africa and have been reported from peoples as distant from each other as the Shilluk of the Upper Nile Valley, the Zulus of South Africa and the Wolofs of Senegal on the Atlantic Coast. Among African kingdoms, those of the Sudan, particularly in its western portion, have been conspicuous for their size, in which they are without rival in Negro Africa. Their early date of historical attestation is only surpassed by Ethiopia and by the kingdoms of the Nile Valley known from Egyptian records. Moreover, the cultural contributions of these Sudanese kingdoms, under the aegis of Islam, are unique in African history. What follows is a brief attempt to consider some of the factors operative in the rise of these empires and to evaluate those theories, found frequently in the literature, which assign a predominant part in their foundation and expansion to Caucasian influences of one kind or another.

The Sudan is the term applied to that vast portion of the African continent which stretches between the Sahara Desert on the North and the tropical rain forests of the Guinea Coast and the Belgian Congo on the south. In keeping with its transitional nature, it is predominantly steppe in its northern portions and savannah towards the south. Rainfall is generally adequate and both agricultural and pastoral populations are found. A conspicuous trait of the area is the relative ease of west-east movement over its open terrain, a facility intensified by the use of the horse as a riding animal, which is at least as old as the introduction of Islam in this region and perhaps older. At the exit of the last century, Rabeh, an adventurer who had been a lieutenant of Zubeir Pasha, active in a rebellion in the Anglo-Egyptian Sudan, fled across the entire eastern and central Sudan and carved out an empire for himself in the shores of Lake Chad. What has occurred so recently in the full light of history may have happened many times in the past also. Certainly, in this ease of communication in the open lands of the Sudan, we have an encouraging factor for the formation of large territorial units.

The great importance of the Sudan in the commercial history of Africa is its position as an entrepôt for goods crossing the Sahara, and for their exchange with the products of the Sudan itself and of the frost area farther south. The Sudan first enters written history, in this guise, with the activities of Mohammedan merchants from the cities of North Africa. Whether this trade first began after Islam had made its conquest of North Africa in the seventh century, or whether it existed even earlier, has been the subject of some speculation. The Romans, to whom is usually assigned the introduction of the camel into North Africa, without which this trade would have been seriously handicapped, if possible at all, are not likely to have bothered to assure themselves of the control of the oases of the Fezzan in southwestern Libya, had not some trade existed. There is no evidence concerning the Carthaginians before them, though some have thought that Carthage maintained such trans-Saharan routes.

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It is clear, at any rate, that the large size of the Negro empires of the western Sudan, their stability over centuries, and the security of travel which they provided, were powerful factors in the development of this trade, while the prosperity which it engendered was, in turn, the *raison-d'etre* for their expansion and wealth. The mere need for such a route is, however, not adequate as an over-all explanation of the existences of these kingdoms. In the seventeenth century, the old organization of the Niger areas in the western Sudan collapsed under the impact of the Moroccan conquest and was never re-established, in spite of the continued operation of the geographical and economic factors which tended to favor this portion of the Sudan. It was the shortness of the route from Morocco, the great bend of the Niger in western Sudan, which led to its early prominence. Islam spread into the interior of Africa along this western path of penetration, trade and religious proselytization going hand-in-hand. The eastern route via the Nile Valley was far less prominent, not only because of its greater length, but because of the persistence of Christian kingdoms in Nubia, which throughout the Middle Ages blocked the southward expansion of Islamic trade and religious conversions.

Our knowledge of the great empires of the western Sudan rests first upon accounts furnished by Arab travelers and by geographers who utilized the reports of Islamic merchants who traded in these regions. Arab materials commence with the tenth century and continue through the Middle Ages. They are sparse in ethnographic detail, but they at least furnish the main outlines of political history. A second source is the Negroes themselves, who borrowed writing and the exercise of historiography, along with other branches of literature, from Islam. The records range from mere lists of kings, with the length of their reigns, to such lively and detailed accounts as the *Tarikh-es-Sudan* of *es-Sa'di*, a native of Timbuctoo. They are an invaluable supplement to the information furnished by Modern European explorers and ethnographers, beginning with the exploration of Mungo Park at the end of the eighteenth century.

The first large kingdom which these sources disclose is that of Ghana, whose existence is attested as early as the tenth century by Ibn-Haukal and is described by El-Bekri, in the eleventh century. The exact location of Ghana is disputed, but it is generally agreed that it was west and north of the great bend in the Niger River, so that it stood as a kind of outpost in the desert for the transmission to the Sudan of goods from the south Moroccan city of Sijilmassa. The rulers of Ghana were pagan throughout its history, although Moslems were welcomed as traders and held important official posts. The capital was divided into two distinct parts, a Moslem city and a pagan city. From El-Bekri's account, it is clear that Islam had spread to the ruling class of other kingdoms of the western Sudan as far west as the Senegal Basin, and eastward to the region of Kukia or Kaokao on the lower Niger, the center of the later Songhoi empire. Ghana became engaged in a struggle with the Almoravids in the eleventh century. These Almoravids were a Berber group from north of the Senegal who, moved by the familiar combination of religious reforming zeal and desire for plunder, finally advanced into Spain, for a time controlling a vast empire, stretching from the Sudan almost to the Pyrenees. They captured Ghana, which subsequently lost most of its commercial as well as political importance although it was not abandoned until the Fourteenth century.

With the decline of Ghana, empire and trade both moved to the east and the hegemony which it had enjoyed passed to the Mandingo empire of Mali. Its racially Negroid rulers spoke a Mandingo dialect, affiliated with the western Sudanese linguistic stock. Its capital, whose exact location is disputed, was certainly somewhere on the Niger, southwest of the great bend. The great commercial center, however, was now Timbuctoo, which, located on the Niger bend, was beginning to establish the reputation for learning and commercial prosperity that made it eventually a name to be conjured with in Europe. The height of its prosperity was attained in the fourteenth century, to which period belongs the celebrated pilgrimage of Mansa Musa to Mecca, recorded by contemporary Arab writers, and the visit of the great Moslem traveler, Ibn Batutah, to Timbuctoo and to the Mandingo capital.

The Mandingo empire, in its turn, entered a period of decline and its position of leadership fell to the Songhoi, formerly a vassal state. In 1468, the Songhoi ruler, Sonni Ali, entered Timbuctoo and under his successor, Askia-El-Hajj, the Songhoi empire became the largest in the history of Negro Africa, extending from the Atlantic almost to Lake Chad and controlling the important oases of the western Sahara. Timbuctoo, also, attained its greatest intellectual importance during the sixteenth century, especially the university associated with Dyingerey Bey, the great Mosque.

In 1590, a trans-Saharan expedition, launched by the Sultan of Morocco, put an end to the Songhoi empire and with it to the prosperity of Timbuctoo and the preeminent position enjoyed for so long by Negro political units in the Niger area. The Moroccan pashas' power soon declined until it was limited to the city of Timbuctoo. The distant ties with Morocco were dissolved and the activities of the Moroccan soldiery, which deposed and set up pashas at will, prevented the re-establishment of public security and the remarkable safety of travel in these regions noted by such earlier travelers as Ibn Batutah.

As a consequence of this situation, the main lines of trade moved more eastward to the Hausa States, a series of city units bound together by language and traditions of common origin for the ruling class, but without any wider political organization. Native records, if their chronology is to be trusted, put the founding of these States in the eleventh century. Subsequently, the rulers of most of them became at least nominally Moslem. For a time, the western Hausa States were under the rule of the Songhoi Empire, when it was at the zenith of its power under Askia-el-Hajj early in the sixteenth century. The great period of prosperity for the Hausa States dates from the Moroccan conquest of Songhoi, after which the route from the great Hausa cities of Kano and Katsina to Tripoli became the chief avenue of trans-Saharan commerce.

The last important political events in the western Sudan before the advent of the European colonial powers involve the people known variously in the literature as Ful, Fula, Peul, Fulani, and Fellata. An essentially pastoral people, they spread, in historic times, eastward from the Senegal basin in scattered groups, often of considerable size, as far east as Wadai, almost half the distance across the entire African continent. All through these regions they lived as politically impotent, often oppressed groups. Some, at least, became enthusiastic and learned Moslem, while others stayed in the bush as pagans or near-pagans. At the beginning of the nineteenth century under the leadership of Osman-dan-Fodio, and a decade later in the Niger basin under Ahmadu, they proclaimed a holy war against the existing political rulers of the Sudan, whom they called pagans. This crusade was, in general, successful and two great confederations were organized, one, in the east, embracing the Hausa States in a loose confederation with its capital at Sokoto, and the other at Hamdallahi in the upper basin of the Niger. These political aggregates were in the process of dissolution when the European colonial powers appeared upon the scene. There were other important political units in the Western Sudan in the period under consideration: for example, the cluster of kingdoms with related rulers, the most important of which was Mossi, in the basin of the Volta River south of the Songhoi Empire. The Mossi remain stubbornly resistant to Islam up to the present day and have formed a bulwark against the spread southward to the frost belt of the Guinea Coast. Another notable kingdom is that of Kanem, later Bornu, on the shores of Lake Chad. It has existed with many vicissitudes from the eleventh century, according to native records. Its ruler was converted to Islam in the twelfth century. At times, this kingdom has been of enormous extent and at one time it controlled a portion of the Hausa States. It successfully resisted the Fulani onrush in the nineteenth century, but finally succumbed to Zubeir Pasha's lieutenant, Rabeh, just before the arrival of the French. In the central Sudan, we find the kingdom of Bagirmi, southeast of Lake Chad, Wadai, east of the same lake, and further east still, Darfur, within the boundaries of the Anglo-Egyptian Sudan. All these kingdoms are authenticated by native records from a more recent period than those of the western Sudan, approximately from the sixteenth century onward, but it is probable that at least small material states existed in this area previously. There are local traditions to this effect almost everywhere.

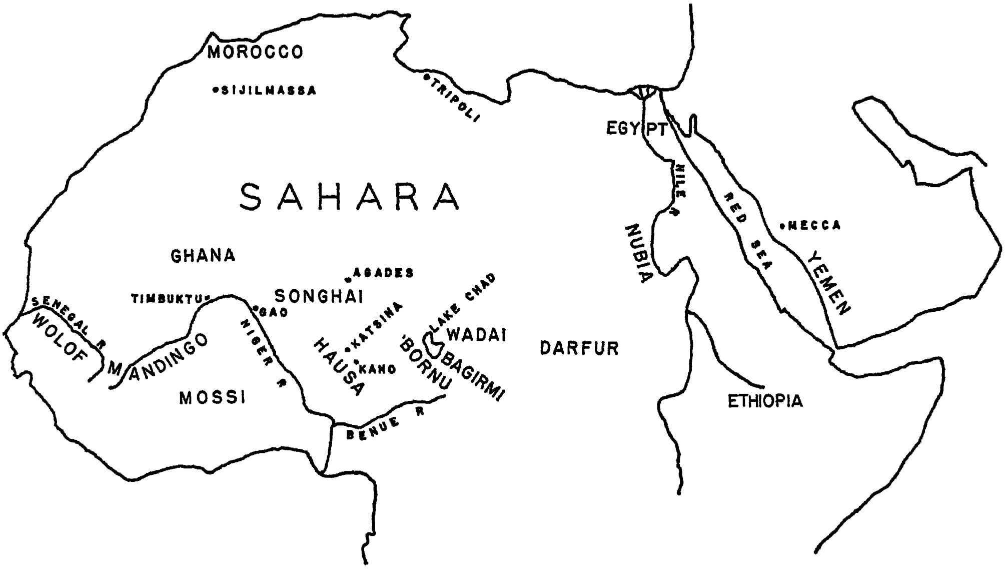

Figure 1

There are a number of theories in regard to the great empires of the Sudan whose history we have just traced in general outline, attributing their origin or their main features to Caucasian influence.

One interpretation in regard to these kingdoms is that they owe their existence to the application of Mohammedan political patterns by racially Caucasoid groups. An early and explicit statement is that of Leo Africanus, the well-known sixteenth century scholar and traveller, who attributes the introduction of political organization in the Sudan to the Berber Moslem Almoravids.

In a statement which forcibly reminds us of the pre-Rousseau conception of the primitive we read:

> Our ancient chroniclers of Africa, to wit, Bishri and Meshudi, knew nothing of the land of the Negroes but only the region of Guechet and Cano, for in their times all other places of the land of the Negroes were undiscovered But in the year of the Hegira 380, by means of a certain Mohumetan which came into Barbary, the residue of the said land was found out, being as then inhabited by great numbers of people which lived a brutish and savage life, without any king, governour, commonwealth or knowledge of husbandry. Clad they were in skins of beasts, neither had they any peculiar wives . . . using hairy skins for beds and each man choosing the leman he had most fancy unto . . . These Negroes were first subject of Joseph, the founder of Morocco, and afterwards under the five nations of Libya [1][*]

Elsewhere, we find less baldly stated the suspicion that these kingdoms are essentially the result of Moslem influence. Thus Delafosse, a standard authority, tells us that there are legitimate doubts concerning the purely Negro character of the great Sudanese kingdoms, by way of contrast with Mossi, which, in his opinion, is purely Negro.[2] The writer in the *Enciclopedia Italiana* mentions "the very interesting hypothesis of the purely Negro origin of these monarchical institutions."[3] P. C. Meyer in his historical survey of the Sudan distinguishes four periods. The first is that of the numerous small heathen kingdoms, which last into the eleventh century, to be followed by the large empires coincidental with the spread of Islam in the Middle Niger.[4] He thus attributes the growth of large states, even if not of political institutions in general in this area, to the influence of Islam.

All these views attributing, in whole or in part, the organization of the large, west Sudanese kingdoms to direct Moslem influences are easily refuted by reference to the known facts about these kingdoms, some of which have already been adduced. On grounds of simple chronological priority, it can be pointed out that each one of them had centuries of stable political organization and wide territorial expansion before the introduction of Islam among the ruling group. For the first of these, Ghana, the evidence is indisputable, inasmuch as its rulers were never converted and it remained pagan throughout its history. So also for Mali, Songhoi, and Bornu the evidence is clear that there was a period of pagan political rule before the introduction of Islam and that the continuity of political institutions was not seriously interrupted by the conversion. The Negro character of Mossi, which was never converted, was, we have seen, emphasized by Delafosse, who is willing to entertain theories of Islamic origin regarding the other states. Indeed, the changes wrought by Islam in the domain of political institutions in this area is very small, and it is striking, that, despite the sparseness of the sources at our disposal, the essentially African nature of Sudanese kingship stands out clearly.

Thus, in the brief account of Ghana by el-Bekri, we find the throwing of dust on the head in greeting the king,[5] the use of royal "interpreters" to repeat everything the king says,[6] the beating of drums to summon a royal audience,[7] the sacrifice of the king's attendants at his death, and sacrifices to the royal ancestors.[8] All of these are clearly African traits and none of them are found in the Islamic kingdoms of North Africa and the Near East. In the Mandingo Empire of Mali, we have likewise the throwing of dust on the head in saluting the king, which struck Ibn Batutah as a characteristic Negro custom,[9] the use of royal "interpreters,"[10] and the important position accorded to the king's chief wife.[11] Similarly in the Songhoi State we find the throwing of dust on the head as a mode of salutation[12] and the office of royal interpreter.[13] We hear on one occasion that a prince regards as a deadly insult the threat on the part of a rival to destroy his drum.[14] This points clearly to the widespread African connection between royal position and the possession of specific drums. In the Songhoi Empire there is present, at least since the time of Askia-el-Hajj, fixed territorial division, each with an associated title and specific privilege and duties, another usual feature of African Monarchical organization absent in the Moslem areas of the Near East. In Bornu, we have, in addition to most of the features already mentioned, the institution of the queen-mother and the queen-sister, each with her own court and officials, which finds its exact parallel in the Uganda kingdom of East Africa.[15]

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[*] By Joseph is meant Yusuf ibn Tashfin, the Almoravid leader. The five nations of Libya are the Berbers. In this division, Leo follows Ibn-Khaldun.

If the evidence for the essentially Negro character of these kingdoms is so clear, one may well ask why there has been so much hesitation or opposition to this view. Outside of the general tendency to deprecate the Negro which permeates the literature on Africa, there are certain superficial data which lend credibility to a theory of direct Moslem origin. For example, the use of the terms "Sultan" and "Vizier" in the literature gives the impression that we are dealing with Moslem political institutions of the Near Eastern type. Our suspicions should be aroused regarding the validity of such deduction when we read that "in Dar-Fur the use of the term Sultan is connected with the possession of certain drums."[16] Sometimes Sultan, Emir and Vizier have been borrowed into a language of the Sudan from Arabic, most frequently as an alternative to an already existing native word. More frequently, when we find such expressions in secondary sources, they have been drawn from Arab accounts in which Sultan would designate any ruler and Vizier any minister. It no more indicates Moslem origin or influence than the title "king of the Zulus" found in an English account would indicate the British origin of Zulu kingship. Occasionally an Arabic term is borrowed and a new office created, but even in this instance, the institutions remain African. Typical in this respect is the Hausa adoption of the word *waziri 'vizier'*, to designate an additional position of nobility. It is but one of a large number of varied titles without any special pre-eminence. The *waziri*, just as other Hausa noblemen, has his territorial fief and fixed prerogatives. The addition of this office has not produced any significant alteration in the existing political system. The only important Moslem influences on political life of which I am aware is the Fulani establishment of an Amiru l-Mu'minina, 'Commander of the Faithful,' on the model of the caliphate, and the office of the Alcali or judge (from the Arabic *qadi*), who judges in accordance with Moslem law in many Sudanese states.

That it is through Islamic influence that writing was introduced into this region and with it a certain development of intellectual life is indisputable. Impressed with the contributions of Negro intellectuals, particularly those of Timbuctoo and Jenne during the Songhoi period, there is a tendency to attribute all developed institutions, whether intellectual, economic, or political, to Moslem influence. However, as the data already adduced indicate, political organization was Negro in structure before Islam and remained so after its diffusion through the Sudan. It is not legitimate to argue from the undoubted Moslem influence on intellectual life to a Moslem origin for political. So, in early Medieval England, intellectual life was Latin and Christian, but political organization was based on Germanic tradition.

Perhaps more than any other factor, the native African traditions regarding the origin and migration of ruling groups have lent credibility to theories of Moslem origin. These reports have not usually been treated with the scepticism they deserve. In spite of Mohammed's admonition that "the noblest among you is the most pious,"[17] the view has always had some currency in Islam that nobility depends upon nearness of kinship to the Prophet. Hence, the constant attempt in the Islamic world to demonstrate descent from the prophet or at least from a well-known Arab tribe or ruling family. For this reason, the accounts of migrations from Arabia and of Arabian descent found among practically all Moslem Negro ruling groups are to be discounted. Thus, the founder of the old Sefawa dynasty of Bornu is said to be the celebrated Saif ibn Dhu Yazan, a pre-Islamic ruler of Yemen in southwestern Arabia. The first ruling house of the Songhoi attribute their origin to the same area. The pre-Fulani rulers of the Hausa states trace their descent from Abu-Yazid of Baghdad. Many other examples could be cited of this general tendency. It is amusing to note what short shrift the Arabist, Basset, gives to some of the most cherished views of Africanists on the subject, in his reply to the African student, Carbou's queries regarding the credibility of these accounts.[18] Whenever the specific individuals mentioned in native tradition as ancestors are known from Arabic literary documents, the evidence is clear that they died in the Near East without ever having migrated to Sudan. Were all African native accounts of this nature true, Near Eastern history would be filled with the exodus of eminent men to the Sudan. But not a trace of these movements is authenticated from Arab sources.

The vogue of these stories in the Sudan is so great that they have on occasion been adopted by non-Moslem Negro groups. The royal house of the pagan, Mossi, tell the same story as their Moslem Songhoi neighbors concerning their origin from Za-Alyamen, a migrant from Yemen in Arabia.[19] It is a corollary of this view of migration from Arabia that in at least two instances, Ghana and Songhoi, we hear of an early dynasty of "white kings." Both of these accounts lack all historic authentication and so must be accepted or rejected along with the accounts of migration from Arab with which they are connected.

Since writing did not exist in this area until the introduction of Islam, all our written accounts come from those areas which are already Islamic and "they try to forget... their predecessors, because they are ashamed of the pagan period."[20] Our narratives are all written by members of the learned class, who are the most enthusiastic Moslems among the population and anxious to develop the theme of the connection with the land and kin of the Prophet.

An allied but distinct point of view does not specifically stress Moslem influence but attributes the establishment of these kingdoms to Negroes with a large Caucasoid racial admixture which supplies the leaven for cultural achievement. This view is clearly stated by A. H. Keane. "Standing out in marked contrast to all these primitive peoples are the relatively civilized Hamito-Negro or Semito-Negro nations, such as the Mandingo, Songhois, and Fulahs in the west; the Hausas, Kanuri, Baghirmi, and Mabas of the Wadai in the centre; and the Furs and Nubians in the east.... From these striking contrasts between the pure Negro and the mixed Negroid peoples, the inference has been drawn that the Negro, left to himself, remains a Negro in every sense of the term, and without miscegenation is incapable of making any advances beyond a low social and intellectual level."[21] Another statement of similar import is that of Meyer that "all these data point to the impulse for the building of States originating from Hamito-Semites who influenced the Negroid elements by mixing with them."[22] Seligman, in his standard work "Races of Africa," describes this process in the following terms: "The mechanism of the origin of Negro-Hamitic peoples will be understood when it is realized that the incoming Hamites were pastoral Caucasians—arriving wave after wave—better armed as well as quicker witted than the dark agricultural Negroes."[23] In the same work, we find the common view that the Bantu languages are the result of a linguistic mixture between a Hamite stratum and a Negroid stratum similar to the Sudanic languages of West Africa, while the Masai and similar pastoral peoples of East Africa are called half-Hamites and stated to reflect an even greater Hamitic linguistic, cultural, and racial preponderance.[24]

It will be observed in these statements that there are both a physical anthropological and a linguistic side to the question. Negroes are declared to be incapable of founding advanced political structures except through physical mixture with racially Caucasoid groups. Such groups are identified with those Negroids or half-Negroids who speak either Hamitic or Semitic languages, presumably imposed on them by Caucasoid speakers of these languages.

With considerable diffidence, inasmuch as I am not a physical anthropologist, I should like to enter a demurrer to the views which dominate the literature on the physical anthropology of the Negro. We are told that only the "forest Negro of the forest zone of the West African Guinea Coast is the true Negro," since it is only here that the dark-skinned, fuzzy haired, broad-nosed and dolichocephalic type is found in his pure form. All other Negroid types are the result of intermixture with Caucasoid peoples, producing such varieties as the Nilote, the Nilo-Hamite, and the Bantu-speaking groups which dominate the southern part of the continent. It would seem just as logical to assert that only the Nordic show the full Caucasoid characteristics of blond hair, blue eyes and tall stature. We could then attribute classical and oriental civilization to Negroid intermixture and state that the leaders of western European civilization—England, France, and Germany—were only so by virtue of admixture with Negroid types producing the Alpine and Mediterranean types found in large or dominant numbers in England, France and Germany.

Even were we to restrict the term Negro to those of the West African tropical forests, we would find such important Negro political structures as the kingdoms of Dahomey, Ashanti, Benin, and the Yoruba states as evidence of the political abilities of the 'pure Negro.'

The linguistic side of this doctrine is notable chiefly for its reliance on an extension of the term Hamitic, particularly by Meinhof, to many languages beyond the established Hamitic groups of ancient Egyptian, Berber, and Cushite languages of Ethiopia, without adequate and convincing proof of linguistic relationship.[25] A conspicuous case is Fulani. The political successes of this people have perhaps been the point of departure, whether consciously or not, for declaring them to be Hamitic. The linguistic evidence, which would take us too far afield to discuss here, clearly points to Fulani being simply another of the vast group of West-Sudanese languages, all spoken by Negroes. It shows special points of resemblance to the Serer language of Gambia and the Biafada of Portuguese Guinea. Again, the view espoused by Meinhof, that the Bantu languages are a result of intimate fusion of Hamitic with languages of the Sudanese 'Negro,' is in a relation of mutual support to the doctrine that the Bantu physical type is the result of admixture between Negroes and Hamitic speaking Caucasoids. Here again the technical linguistic problems cannot be treated adequately at this point. However, languages do not 'mix' after this fashion, or at least such views are looked upon with extreme suspicion by linguists as unusual and requiring real proof, which has not been offered in this case. The clear fact seems to be that the Bantu languages are related to those of the Western Sudan and that the 'Hamitic' features are illusory.[*] Likewise, the Masai and other so-called half Hamitic languages are not Hamitic at all, but clearly related to the Nilotic languages of the Shilluk and other tribes of the White Nile as well as the Nuba farther north.[**]

The independence of language and physical type is a fundamental theoretical achievement of contemporary anthropology and there is no wish to sacrifice it here. It is interesting to see, however, that if we grant the premise that Hamitic and Semitic speech are evidence of Caucasoid racial descent, we can then ask what has been accomplished by peoples of the Sudan who speak indisputable Hamitic and Semitic languages. If infusion of the precious

[*] This relationship has been noted particularly by D. Westermann without affecting standard doctrine regarding African linguistic relationship. Proof of this extremely obvious connection will soon be published by the writer.

[**] This was clearly demonstrated by G. W. Murray (1920) in a common-sense demonstration.

Hamitic and Semitic 'blood' has such civilizing effects on the Negro, it should be most manifest in those groups which speak languages of the Semitic and Hamitic group. Actually, the record is most uninspiring. The Tuareg, a Berber Hamitic group, has been present in the Niger bend region since the Middle Ages in large numbers without overcoming surrounding Negroid groups, or founding any political structure of note. The Moroccans, Semitic-speaking Caucasoids, as we saw, conquered the Songhoi Empire, but brought in its stead chaos. The Tunjur Arabs, found throughout the central Sudan, were apparently either pagan or very loosely Moslem Arabs, and yielded political sovereignty to the Mohammedanized Negro people of the Fur in Dar-Fur and the Negro Maba of Wadai. Other Arabs of the Sudan, for example, the Shuwa of Lake Chad, have been politically impotent and contributed scarcely anything to Islamic culture. All this is not meant to prove that Hamitic and Semitic speaking peoples in Africa are incapable of political organization, but to demonstrate the *reductio-ad-absurdum* of the conventional arguments.

Our conclusion, then, is that the great empires of the Sudan were instances of a widespread Negro pattern of political structure and that, stimulated by the trans-Saharan trade and taking advantage of the open terrain afforded by the Sudan, territorial organization on a wide scale and of considerable stability became possible.

Finally, the remarkable degree to which Negro institutions survived under a Moslem exterior may be documented from the example of Dar-Fur.[26] Although under Moslem rulers since approximately 1600, all of the characteristic features of Negro kinship already enumerated were found in a flourishing condition by Nachtigal in the middle of the nineteenth century, although the country was then raided by the Mohammedan governor of the Sudan. There was a keeper of the sacred stool, as in the West African kingdom of Ashanti. The Queen Mother had her own troops and courtiers. An aboriginal compilation of customary law was in alternative use in place of the Moslem Maliki law. The great annual festival was the feast of the drums in the Mohammedan month of Rajab. On this occasion, the slaves devoted to each of the past kings sacrificed animals to the royal souls. For those kings who were Mohammedans, learned men read chapters of the Koran, while, for the ancient pagan line, all proceeded to a pre-Islamic sacred place at Jebel Kora where animals were sacrificed to the pagan royal ancestors.

The skins of the slain animals were employed to cover anew the royal drums for the succeeding year. These are just a few of the observances which convince us that the institution of Darfur resemble those of Ashanti, Dahomey, Uganda, and other Negro kingdoms in a thoroughgoing fashion.

There are indications that the traits of Negro political structure also continued with great persistency under a Christian façade in Ethiopia and under Egyptian influence in ancient Cush and Meroe.*

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* See, for example, the account of king-killing in Diadorus's account of the Nubian Kingdom of Napata (iii, 6) King-killing, a widespread African trait, is unlikely to have been borrowed from Egypt in this instance, since it was already abolished in the early dynastic period, more than two thousand years earlier, and is postulated by Egyptologists from such survivals as the Sed-festival.

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SECTION OF PHYSICS AND CHEMISTRY

APPLICATION OF REACTION RATE PRINCIPLES TO SOME MECHANICAL PROPERTIES OF MATERIALS

By EDWARD SAIBEL*

Introduction. It is well known that, as science progresses, the boundaries between what were formerly isolated and discrete phases of a subject become less and less distinct, and it becomes more difficult and less meaningful to compartmentalize the treatment of problems.

A decade ago such phenomena as creep, viscosity, and fatigue, to mention a few, were considered purely mechanical and were treated as such. That this treatment was inadequate is evident from the fact that the work done on those topics during that period consisted mainly of the accumulation of masses of unrelated and uncoordinated data. Experiments were performed in more or less random fashion, and many variables went uncontrolled because, either no guiding theories had been proposed to develop systematically what those variables were, or the theories proposed were too restricted to account for all the known phenomena.

Much remains to be done and we have by no means found all the answers. Nevertheless, broad unifying principles have been established, and such subjects as the ones mentioned are now amenable to some kind of systematic treatment. No longer is it necessary to separate chemical from mechanical behavior, with the result that such studies as, for example, the effect of the atmosphere on the fatigue life of metals or glasses may now be treated in a rational manner.

Most mechanical phenomena have not yet been successfully attacked from a molecular point of view; that is, detailed mechanisms have not been set up from which rigorous quantitative analyses may be developed. Noteworthy examples are the fields of plastic flow and fracture. The approach has been more successful from the energy or thermodynamic point of view. The results from the latter treatment have been semi-quantitative in most cases, and adjustable constants have been left in the derivations. But trends have been noted, variables or parameters upon which the system depends have been recognized, and in some cases absolute relationships have been established.

In lieu of the detailed mechanism of a process, it is often possible to set up some model, not too complex, in which units of perhaps unspecified components or size perform in a certain way. Since, in general, any system may be considered as consisting of a large number of such units, a statistical approach is indicated. Such a method, based on an earlier principle of Arrhenius, has been developed by Eyring and his co-workers.[1] In recent times, other statistical methods and approaches have been developed, as for example, that of Barrer,[2] but for the time being the treatment discussed will be that of Eyring, and this will serve to illustrate the general method and its applications. Even though many valid criticisms have been leveled at the Eyring theory, it still remains the only quantitative one which is workable in most physical situations.

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*Department of Mathematics, Carnegie Institute of Technology, Pittsburgh, Pennsylvania. This paper, illustrated by lantern slides, was presented at the meeting of the Section on May 4, 1948, and has been submitted for publication at this time. There was no meeting of the Section in January, 1949.

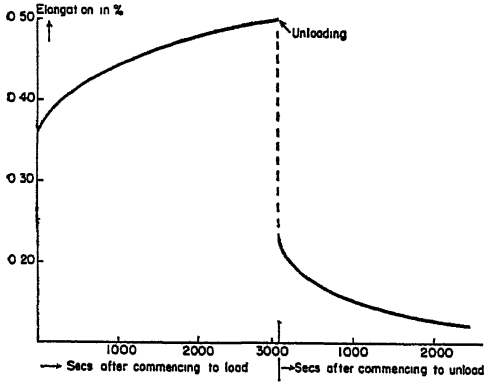

FIGURE 1

General Considerations. Let us consider, for example, what happens to a solid when it is subjected to forces, FIGURE 1.[6] Let us suppose that these forces are applied gradually and that in a macroscopic sense the body is in statical equilibrium, so that no extraneous complexities are introduced into the problem. In some materials and under certain conditions, when the externally impressed forces are small, the deformations may be considered proportional to the forces. When the forces are released, the body returns to its original shape, or at least closely enough to the original shape so that permanent strains are undetectable by the most sensitive of present instruments. We know that, strictly speaking, there must always be permanent changes in a body; because no body is homogeneous. In all cases, there are many sources of stress concentrations within it, and, no matter how small the load, there will be places where the stresses developed through these "stress-raisers" will far exceed the average stress developed. Thus, there will be many places of permanent strain, but so highly localized that for some purposes they may be neglected, particularly in the macroscopic treatment of the body.

As the load is increased, the next stage may be termed the transient stage. Here deformation takes place at a slow and decreasing rate with time. If the loads are removed, the substance still returns, as far as can be determined, to its original shape. But unlike the elastic process, which is extremely rapid, this movement takes place slowly. This effect is called "delayed elasticity" or "elasto-viscous flow." During recovery it is sometimes termed "elastic after-effect."

In the third stage, a steady flow takes place. If the rate of flow is proportional to the stress we have "Newtonian Flow," otherwise, "Non-Newtonian Flow." Such steady flow always leads to permanent deformation. Transient and steady flows take place at measurable and comparatively slow rates. These flows are strongly temperature dependent and are amenable to the general treatment of physical transport processes requiring activation energies.

In the present treatment, the units of flow may be thought of as being situated in an energy field. The equilibrium positions are the potential minima. In general, there will be more of these minimum positions than there are units of flow, and for a unit of flow to move from one of these positions to another, it is necessary to surmount the potential energy barrier. The energy required to do this is called the energy of activation. There is, of course, a certain amount of thermal motion in the system, the amount depending on the temperature, and there will be some movement over the barriers due to this thermal agitation, but on the whole the distribution of units in space remains constant unless the system is subjected to a potential field. This field may arise from many causes—it may be due to an applied force, or it may be due to a chemical or electrical potential, *etc.* One of the major advantages of the method lies in the fact that it is not necessary to specialize the cause of the disturbance. Another advantage comes about from not having to specify the exact nature of the unit of flow.

Reaction Rate Theory. The process of unit flow is treated in the same manner as a chemical reaction. In its state before flow it is the reactant, and after the flow has taken place its state is defined as the product of the reaction.

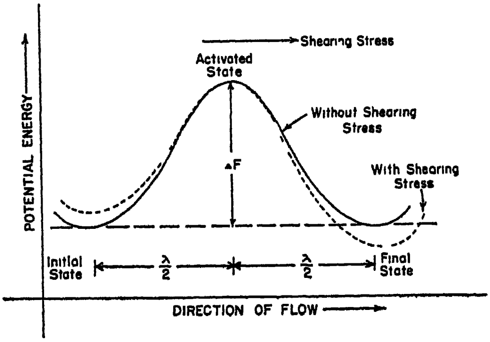

FIGURE 2

In order to go from one state to the other, it is necessary for the flow unit to attain the energy of activation ΔF*, FIGURE 2. From statistical mechanical considerations,[1] it may be shown that the specific rate of the reaction in the forward direction is given by

$$n_f = \frac{kT}{h} e^{-(\Delta F^*/RT)} \qquad (1)$$

where k is Boltzmann's constant 1.38×10^{-16} *erg deg*,$^{-1}$
T is absolute temperature,
h is Planck's constant 6.62×10^{-27} *erg-sec*,
ΔF^* is the free energy of activation or change in free energy in going to the activated state. In other words, it is the energy necessary to get over the potential energy barrier,
and R is the gas constant.

The specific rate in the backward direction is likewise

$$n_b = \frac{kT}{h} e^{-(\Delta F_*/RT)}. \qquad (2)$$

In the absence of a potential or force field the net change is zero, since n_f and n_b are just equal. However, if a force is applied so that the work done on a flow unit per jump is w, then

$$n_f = \frac{kT}{h} e^{-(\Delta F^*/RT)} e^{u/kT} \qquad (3)$$

and

$$n_b = \frac{kT}{h} e^{-(\Delta F^*/RT)} e^{-(w/kT)}. \qquad (4)$$

The net result, n, is now

$$n = n_f - n_b$$
$$= \frac{kT}{h} e^{-(\Delta F^*/RT)} \left(e^{w/kT} - e^{-(w/kT)}\right), \qquad (5)$$

or using the definition of the hyperbolic sine, $\sinh u = \frac{1}{2}(e^u - e^{-u})$, EQUATION 5 may be written

$$n = \frac{2kT}{h} e^{-(\Delta F^* RT)} \sinh \frac{w}{kT}. \qquad (6)$$

The net velocity of forward motion is given by the net number of jumps per second multiplied by the distance moved by the flow unit in each jump, λ, thus

$$v_{\text{net}} = \frac{2\lambda kT}{h} e^{-(\Delta F^*/RT)} \sinh \frac{w}{kT}. \qquad (7)$$

Several important cases arise which we shall treat separately.

Case I $w \ll kT$

Physically this means that the work done is small in comparison with the thermal energy. Since for small values of u, $\sinh u \approx u$, we may write

$$v = \frac{2\lambda kT}{h} e^{-(\Delta F^*/RT)}$$
$$\therefore \frac{w}{kT} = \frac{2\lambda w}{h} e^{-(\Delta F^*/RT)}. \quad (8)$$

The work done, w, may be expressed in the form $\frac{f\lambda_2\lambda_3\lambda}{2}$ where f is the shearing stress on an element of area $\lambda_2\lambda_3$ and acting through the distance $\frac{\lambda}{2}$ from the initial state to the top of the energy hill. Thus

$$v = \frac{f\lambda_2\lambda_3\lambda^2}{h} e^{-(\Delta F^*/RT)}. \quad (9)$$

However, by definition

$$\text{viscosity} = \frac{f}{(\text{velocity})/\lambda}$$
$$= \frac{h\lambda}{\lambda_2\lambda_3\lambda^2} e^{\Delta F^*/RT} \quad (10)$$

λ being the distance between layers.

The three λ's are approximately equal, their product is approximately the volume of the flow unit, so we may write,

$$(\text{vicosity})\ \eta = \frac{Nh}{V} e^{\Delta F^*/RT} \quad (11)$$

where V is the volume of a flow unit, and N is Avegadro's number. Taking the logarithm of this equation it is apparent that, if the relation is true, log viscosity plotted against $\frac{1}{T}$ should give a straight line. This is shown in FIGURE 3, taken from Houwink[6].

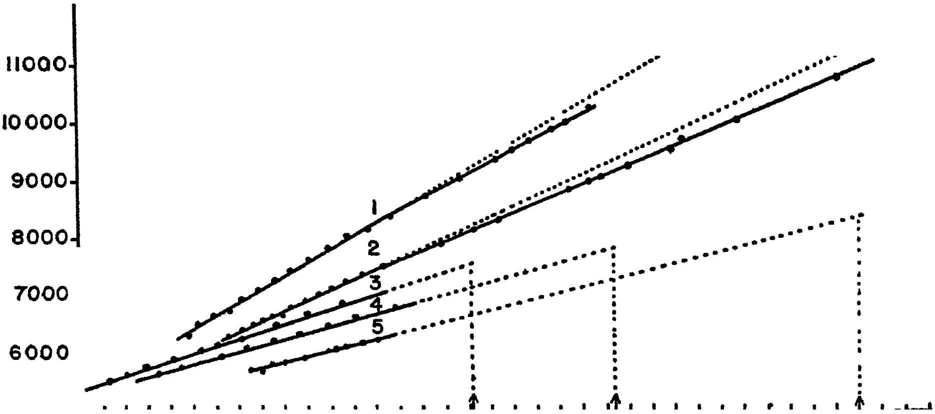

FIGURE 3 Curve (1) n-prophyl-alcohol, (2) ethyl-alcohol, (3) o-xylene; (4) n-octane; (5) methyl-acetate.

This kind of flow, in which velocity is proportional to the shear force, is termed "Newtonian Flow."

Since

$$\Delta F^* = \Delta H^* - T\Delta S^* + p\Delta V^* \quad (12)$$

the dependence of viscosity on pressure may be brought out when the equation for the viscosity is written in the form

$$\eta = \frac{Nh}{V} e^{-(\Delta S/R)} e^{(\Delta H/RT)} e^{p\,(\Delta/RT)} \quad (13)$$

where ΔH^* is the heat of activation
ΔS^* is the entropy of activation
ΔV^* is the volume change during activation
and p is the pressure

The volume change during activation may be interpreted as the volume of the hole which must be provided for a molecule or unit to flow. For a number of simple liquids this is about $\frac{1}{7}$th or $\frac{1}{8}$th of the volume of a molecule, while for molten metals it is about $\frac{1}{8}$th of the volume of a metal ion. The effect of pressure is shown in FIGURE 4, taken from Eyring and Powell.[4]

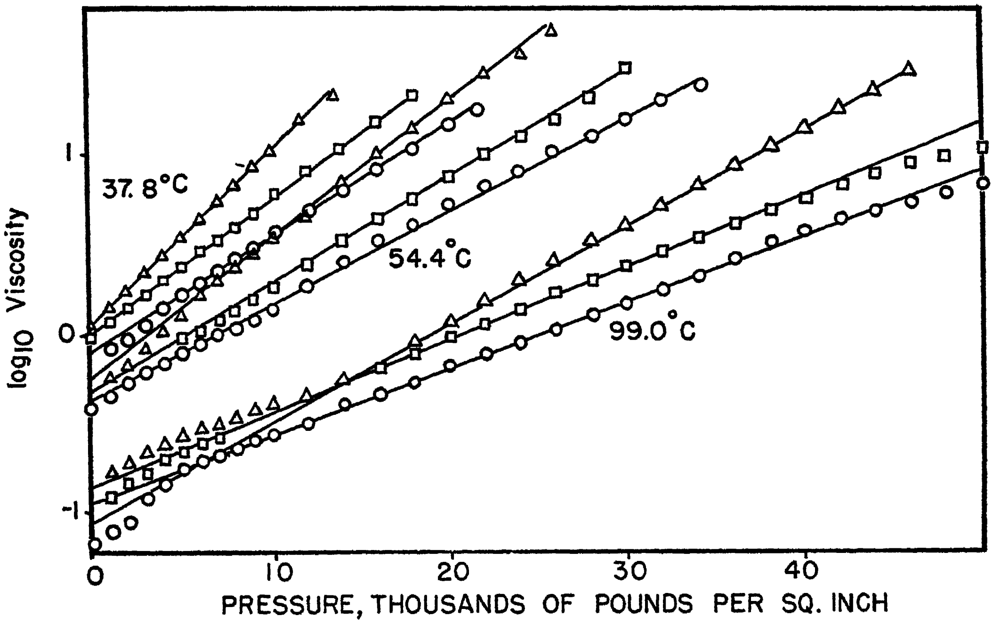

FIGURE 4. Effect of pressure on the viscosity of lubricating oils. (Circles correspond to Pennsylvania oil, squares to Oklahoma oil, and triangles to California oil)

Case II w ≫ kT

When the work done by the field is extremely large in comparison with the thermal energy, the free energy necessary to reach the top of the energy hill must also be large compared to kT. The basic equation may be written

$$w = \frac{\Delta F^*}{N} - kT \ln \frac{\lambda kT}{vh}. \qquad (14)$$

This is obtained by using *sinh u* $\approx \frac{1}{2}e^u$ for large u. When the second term may be neglected in comparison with the first, all the work necessary to reach the top of the potential hill must be done by the applied force. This amounts to $\frac{\Delta F^*}{N}$ per molecule. Note that this is the value approached as T approaches absolute zero. In this state no thermal energy would be present and no motion would take place until the critical stress $f = \frac{\Delta F^*}{V}$ was reached. A general relationship between yield stress and temperature may be deduced.[5]

$$f_y = A - BT \qquad (15)$$

where B is approximately independent of T. This may be contrasted with the same type of relationship developed by Becker and Orowan.[6]

$$f_y = A - B\sqrt{T}. \qquad (16)$$

Unfortunately, sufficient data are not available for a critical appraisal of the two expressions. This type of equation has been applied to the sliding of one body on another under dry friction. Here, too, a yield point phenomena is observed: that is, up to a certain critical load no motion takes place;

beyond that critical value, the system moves.[7]

Case III Here w and kT are of the same order of magnitude, and the general expression for the velocity is given by

$$v = \frac{2kT\lambda}{h} e^{-(\Delta F^*/RT)} \sinh \frac{w}{kT}. \quad (17)$$

This relationship has been applied to the problem of creep in metals,[8] although in many cases it is sufficiently accurate to use the formula of Case II

$$v = \frac{kT\lambda}{h} e^{-(\Delta F^*/RT)} e^{w/kT} \quad (18)$$

Shear rate =

$$\frac{v}{\lambda_1} = \frac{kT\lambda}{h\lambda_1} e^{\Delta F^*/RT} e^{f\lambda_2\lambda_3\lambda/2kT} \quad (19)$$

where λ is the average distance between the layers, which slide relative to each other. This leads to

$$\log (\text{shear rate}) = A + Bf. \quad (20)$$

EQUATION 20 may be seen to hold approximately under certain conditions in FIGURE 5, taken from work of Nadai.[9]

In recent years, applications of the methods of reaction-rate theory have been made to various other problems. In what follows we shall limit ourselves to a few examples in which applications to mechanical problems and the question of the interpretation of the quantities entering into the expressions are involved.

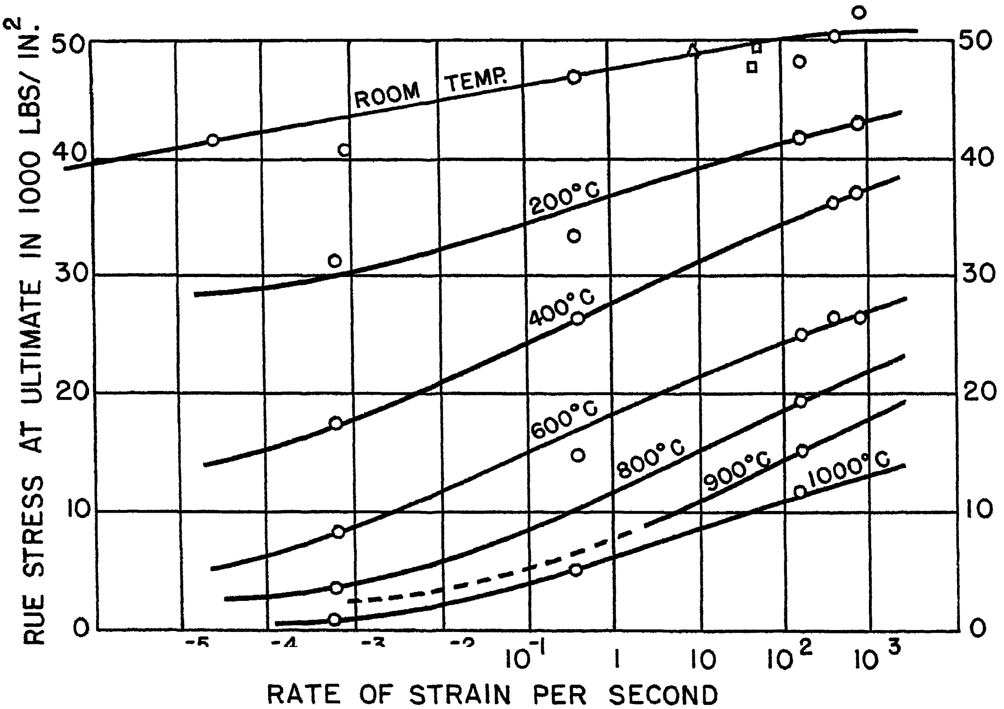

FIGURE 5. Effect of strain rate on true stress at ultimate of pure copper at various temperatures.

Speed of Propagation of Fracture Cracks. The phenomenon of the so-called "brittle fracture" of metals, in particular of steel, has been the subject of much attention in recent years, principally because of its connection with the catastrophic failure of welded cargo vessels.[18] One of the characteristics of this type of rupture is the extreme speed with which the crack is propagated along the plate. We have seen that the velocity of a reaction may be expressed in the form given by EQUATION 18. This equation will be used to determine the rate of propagation of a fracture crack in the following manner:

Sometime ago, the author[10] developed a thermodynamic theory of the fracture of metals in which fracture was related to the melting phenomenon, use being made of the hole theory of the liquid state. The abolition of cohesive strength was attributed to the formation of the hole structure. It was shown that the fracture took place when the critical energy density u_c reached the value

$$u_c = \left(L \frac{\Delta V}{V_s} + p\right) \qquad (21)$$

where L is the latent heat of melting,
ΔV is the change in molecular volume on passing from the solid to the liquid state,
V_s is the molecular volume of the solid at the melting point,
and p is the hydrostatic component of the force system acting at the point of formation of a hole. If this component is compression, p is to be taken as positive.

The speed of fracture will be determined from the rate at which the holes are formed. The metal just before rupture is considered the reactant, and the fracture crack which is formed is taken to be the product of the reaction, so that, in the present interpretation, the substance is in the activated state when the holes are formed. Thus, w is defined as the energy necessary to form one hole. These holes will be thought of as being spherical and of an average uniform size. The mechanism of crack propagation will be assumed to take place through the continuous formation of holes. Each time a hole is formed, the distance advanced by the crack will be equal to the diameter of the hole and will correspond to λ in the fundamental equation. The critical condition for the energy density will be assumed satisfied so that crack propagation will continue. The evaluation of the terms entering in the equation for the velocity is as follows: we may express the free energy of activation in the form

$$\Delta F^{\ddagger} = \Delta H^* - T\Delta S^* \qquad (22)$$

if the hydrostatic component of stress be neglected. In order that fracture take place the energy corresponding to the heat of activation must equal the critical energy of EQUATION,[21] thus

$$\Delta H^* = u_c. \qquad (23)$$

We assume, furthermore, that the work done in forming the holes is equal to the heat of activation. Now

$$\frac{w}{kT} = \frac{Nw}{RT} = \frac{N}{n_h} \cdot \frac{n_h w}{RT} = n \frac{\Delta H^*}{RT} \qquad (24)$$

where $n = \frac{N}{n_h}$.

We may now write the equation for the velocity of propagation in the form

$$v = \frac{2\lambda kT}{h} e^{(\Delta S^*/R)} e^{-(\Delta H/RT)} \sinh \frac{n\Delta H^*}{RT}. \qquad (25)$$

The entropy change of activation for the process is calculated by considering the liquid to be a random mixing of molecules and holes[11]. This leads to the expression

$$\Delta S^* = Nk[(1+\gamma)\ln(1+\gamma) - \gamma \ln \gamma] \qquad (26)$$

where $\gamma = \frac{1}{n} = \frac{n_h}{N}$.

Also $\Delta V = n_h \cdot \frac{1}{q} \frac{V_s}{N}$, if it be assumed that the whole volume change on melting be due to the formation of holes and that the volume of a hole V_h be defined as $\frac{1}{q}$ times the effective volume of a molecule $\frac{V_s}{N}$.

Thus $\frac{1}{n} = q\frac{\Delta V}{V_s} = \gamma$, and from a knowledge of $\frac{V}{V_s}$ and of γ, q may be found. The value of n may be taken to be 2^1, in accordance with known entropy values, as well as other evidence. Furthermore, for Fe, $\frac{\Delta V}{V_s} = 0.045$.[12] From this it follows that q is approximately 11. Using this value and $V_h = \frac{1}{q}\frac{V_s}{N} = \frac{4}{3}\pi\frac{d_o^3}{8}$, we find that the average diameter of a hole, $d_o = 1.28 \times 10^{-8}$ cm. The heat of activation $\Delta H^* = L\frac{\Delta V}{V}$ is equal to 160 cal/mol and $\Delta S^* = 0.95$ R. From these numerical data the speed of propagation v becomes

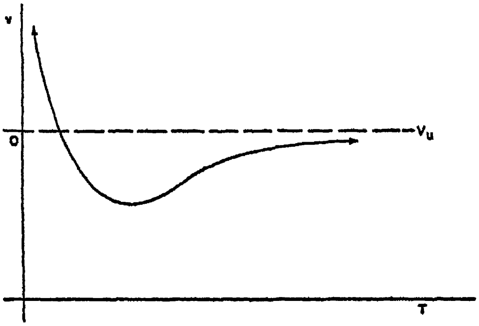

FIGURE 6

$$v = 542Te^{-80/T}\sinh\frac{160}{T}\text{ in/sec.} \qquad (27)$$

At room temperature, $T = 300°\ K$, and $v = 69{,}500$ in/sec. Hudson and Greenfield[13] find an average experimental value of 40,400 in/sec.

It is interesting to note that no adjustable constants enter in this calculation, although it may appear that the value of n may be varied. This value is determined, however, by the known value of the entropy of melting. The fact that the calculation gives a somewhat high value may be explained from the assumed straight line motion of the crack. Actually, although not random in character, it zigzags through the metal, and the net macroscopic speed is slower than the calculated speed. Furthermore, no account was taken of the hydrostatic component, which may well effect the results.

If the speed of fracture be written in the form

$$v = ATe^{-(\Delta H^*/RT)}\sinh\frac{n\Delta H^*}{RT} \qquad (28)$$

where A is independent of temperature, two things become apparent, FIGURE 6. First, there exists a minimum speed at a certain temperature. This is found to be when

$$\tanh nx = \frac{nx}{1+x} \qquad (29)$$

where $$x = \frac{\Delta H^*}{RT}.$$

And secondly, as the temperature increases, the speed of fracture approaches, asymptotically, a limited value. This limiting speed seems to be the speed of propagation of sound in the liquid at the melting point. For example, using EQUATION 28 and allowing T to become large, we find

$$v_u = \frac{n\Delta H^*}{R}\frac{2k\lambda}{h}e^{\Delta S^*/R} \qquad (30)$$

which for iron gives 94,400 in/sec, and for copper 70,200 in/sec. The speeds of sound in these two materials in the liquid state and at the melting point are approximately 100,000 in/sec and 70,000 in/sec, respectively.

Static Fatigue of Glass. Another problem of much interest is that of the strength of glass under a static load. As early as 1899, Grenet observed that a glass specimen which would withstand the application of a load for short periods of time would fail under smaller loads which were applied for longer periods. Since that time, a great deal of experimental work has deen done and tests of duration ranging from 10^{-2} sec to 10^6 sec have been carried out. Many explanations of the phenomenon have been advanced and many empirical formulae have been shown to fit the data about equally well. Typical graphs of the relationship of load and time to fracture are shown in FIGURES 7 and 8, taken from REFERENCES 14 and 15 respectively, the former being for glass and the latter for rayon used in tire cord. As a matter of fact, there seems to be a wide class of materials to which the same general principles may be applied and for which the general relationship of time to fracture and load holds. In all of these cases, the effect of the surrounding atmosphere is important and may be taken into account in the treatment that follows, FIGURE 9.[16] The effect of the atmosphere is also apparent in FIGURE 7, where the curve has been lowered by both the scratching of the glass and the wetting of the surface.

As is well known, the structure of glass is irregular, like that of a liquid. There is no repeating regularity, although there is some degree of order, in any small neighborhood. The model on which we shall base our calculations is capable of several interpretations. The simplest case, SiO_2 in the amorphous state, has each silicon surrounded by four oxygens and each oxygen by two silicons in a sort of distorted tetrahedral arrangement. One may think of a distribution of primary *Si-O* bond strengths existing in any neighborhood due to missing bonds, cracks, holes, *etc.* Or one may imagine this distribution of strength as due to initial strains of varying amounts in the bonds filling the elemental region, the size of which is as yet not determined.

Parenthetically, it may be pointed out that, since the average strength of the primary bonds is rather large, it has been suggested that it is not these bonds which are broken, but rather the Van der Waals or secondary bonds, which are postulated to act between aggregates or regions of the material.[6] Because these forces are much weaker than the primary forces, some have asserted that it is more reasonable to set up the fracture process in terms of the secondary bonds. Evidence of the existence of these bonds is lacking, however, and as we shall see, it is not necessary to specify in detail the nature of the fracture mechanism for our purposes. It will be sufficient to assume a distribution of bond strengths and to show that out of it a relation between fracture stress and time develops, which correlates with the experimental evidence and may, in fact, be used to determine the nature of the bond strengths themselves.

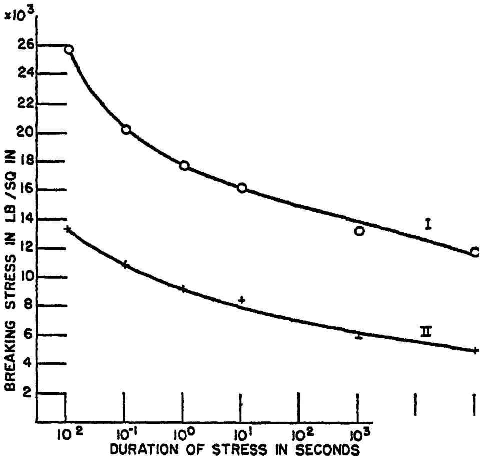

FIGURE 7. I Disannealed Pyrex, dry, II. Disannealed Pyrex, scratched, wet

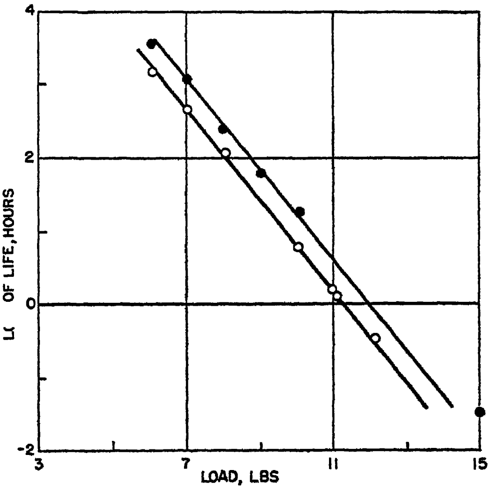

FIGURE 8 Life under dead load as a function of load for cotton tire cord (after Busse et al).

In the following treatment we shall consider the glass before fracture as the reactant; and broken bonds, the product of the reaction. As we have seen before, the rate of the reaction, n, may be expressed

$$n = A \sinh B f \qquad (31)$$

where f is the force acting on a bond, related to w though $w = \frac{f\lambda}{2}$, λ being some characteristic distance through which the force f acts in breaking a bond. If N is the number of bonds broken, this basic equation becomes

$$n = \frac{dN}{dt} = A \sinh Bf. \qquad (32)$$

The force f acting on a bond may vary with time for several reasons:

a. The load acting on the specimen may be changing.
b. As bonds are broken, the remaining ones have to assume larger loads.
c. The directions of the bond elements may be changing.

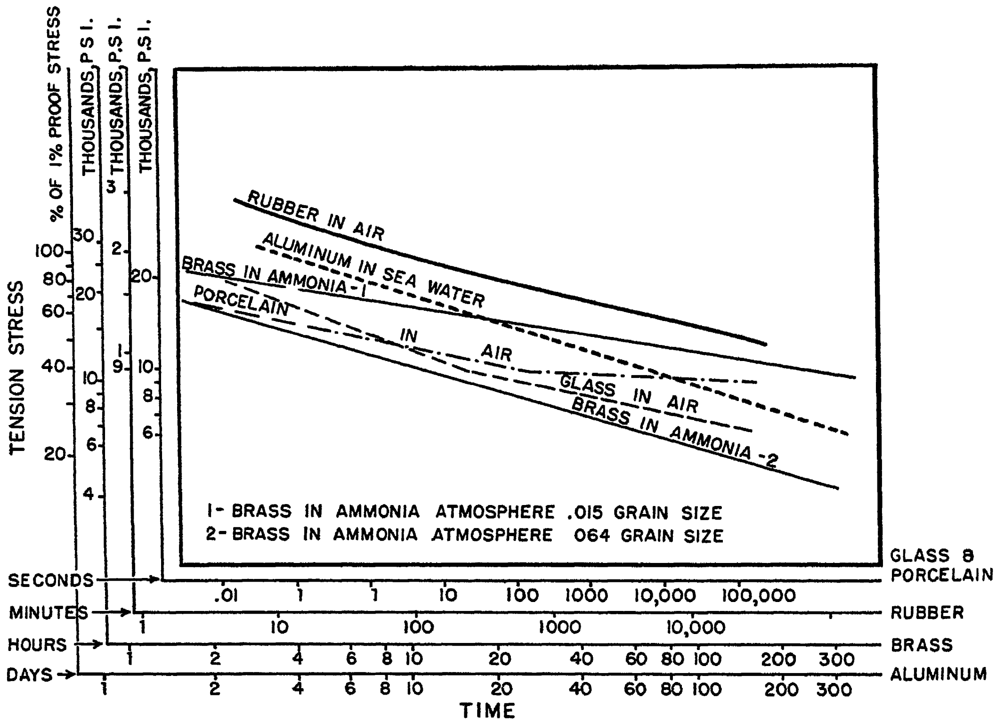

Figure 9*

We consider the case where the load is applied in a steady manner, as for example, constant tension in the macroscopic sense. As a first approximation assume that

$$f = fo + cN \qquad (33)$$

where c is a constant and fo is the force on a bond when the load is first applied. Then

$$\frac{dN}{dt} = A \sinh B(fo + cN). \qquad (34)$$

Integrating and assuming that the number of bonds which must be broken to bring about fracture is large, we find

$$t_B = \frac{1}{ABc} \log \coth \frac{B}{2} (fo + cNo) \qquad (35)$$

where t_B is the time to fracture

N_B is the number of bonds which must be broken to bring about fracture in the usual sense.

---

* Sources of materials used glass and porcelain from Preston-Preston Laboratories; rubber from Sloman-U S. Rubber; brass from Morris-Bridgeport Brass Co ; aluminum from George & Chalmers—Brit Iron & Steel Inst.

No is the number of bonds already broken at the start of the test $t = 0$.

This in turn may be inverted to yield

$$B(fo + cNo) = \log \coth \frac{ABc}{2} t_B \qquad (36)$$

where

$$A = \frac{2kT}{h} e^{-(\Delta F^*/RT)}$$

and

$$B = \frac{\lambda}{2kT}.$$

If we assume that $No = 0$, EQUATION 36 may be written

$$f = D \log \coth E t_B. \qquad (37)$$

D and E are parameters independent of the applied force. The general shape of the curve representing EQUATION 37 is shown in FIGURE 10, where because of the large range in values of t which must be covered, the fracture stress is plotted against the logarithm of time. Notice that over a fair sized range the curve is nearly straight, since for small values of the argument, $\coth x \approx \frac{1}{x}$. Thus

$$f = D \log \coth Et \approx -D \log Et. \qquad (38)$$

On the other hand when the argument of the hyperbolic cotangent is large,

$$\coth x \approx 1 + 2e^{-2x}$$

and $\log \coth x \approx 2e^{-2x}$ so that

$$f \approx 2De^{-Et} \text{ for large } t_B. \qquad (39)$$

This latter expression invites comparison with the equation of relaxation of tension with time in a material stretched to some value just below the breaking load and held at constant length. It is well known that under these conditions the load P varies with time t in the following manner for some materials

$$P = A\, e^{-Bt} \qquad (40)$$

which is precisely the form developed above. In some respects the two problems are comparable, for if the load is held just below the breaking point one might well imagine that this load behaves with time in the same way as the breaking load does.

No attempt has been made in this treatment to obtain the absolute values of the constants entering into the basic formulae. It is evident that their values will depend on the nature of the atmosphere and the resulting chemical action that takes place. This will manifest itself in general through a lowering of the potential energy barrier and a consequent lowering of the mechanical force necessary for fracture in a specified time.

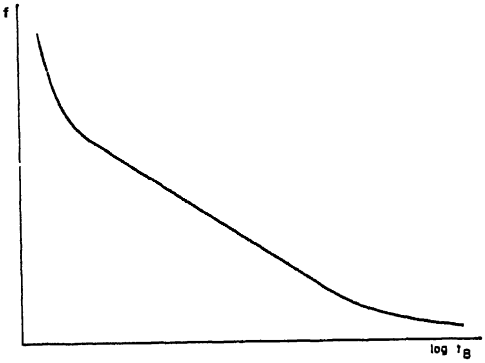

FIGURE 10 $f = D \log \text{Coth}\, Et_B$

The simple picture of a substance consisting of numerous similar units does not always lead to a satisfactory solution of a problem.

An example of this arises in the attempt to account for the variation of the viscosity of glass with temperature. Recall that, from the Eyring point of view, log viscosity vs. $\frac{1}{T}$ should be a straight line. However, as can be seen from FIGURE 11, which is characteristic, such is not the case. The modification as performed by Powell[3] is to assume two molecular species, miscible and in equilibrium with each other. These are designated as a low temperature form (LT) and a high temperature form (HT), and the composition of the mixture is supposedly given by

$$\frac{(HT)}{(LT)} = \text{Kequ.} = e^{-(\Delta Fo/RT)} \qquad (41)$$

where ΔFo is the free energy change of the reaction. It is also assumed that the viscosity of mixtures is given by

$$\eta = \frac{Nh}{\text{Vave}}\, e^{(\Delta F_{ave}/RT)} \qquad (42)$$

where $Fave = N_1 \Delta F_1 + N_2 \Delta F_2 \ldots$; N_1, N_2, ... being the mole fractions of the mixture. This means that the free energy of activation required for the flow process may be expressed as a weighted average. EQUATION 42 may be put in the form

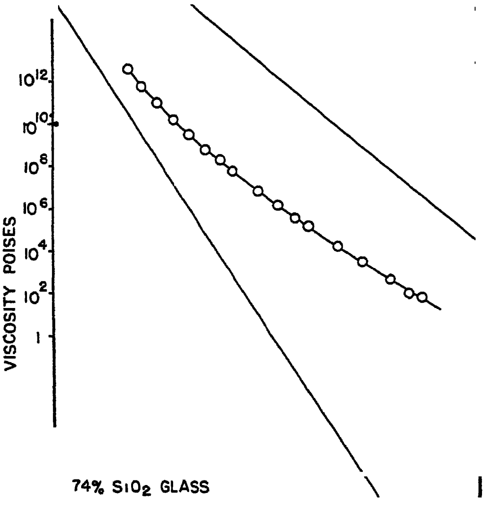

FIGURE 11

$$\log(\text{viscosity}) = N_1 \log(\text{viscosity}_1) + N_2 \log(\text{viscosity}_2) + \ldots \qquad (43)$$

The agreement between theory and experiment for the viscosity of glasses is then found by assuming two straight lines in the $\log \eta$ vs. $\frac{1}{T}$ diagram, so that the combination results in the experimental curve.

This may be compared with the concept of Murgatroyd,[17] who postulates a diphase material in order to account for the time effect in the fracture of glass. He envisages a quasi-viscous material in an elastic matrix. When the glass is stressed, relaxation in the viscous pockets throws more of the load on the elastic portion, and, after a certain length of time, fracture occurs. Of course, it is not necessary to impose a limit in this concept to just two components; any number may be postulated. In fact, some advantage may be gained by assuming an infinite number and by determining the relaxation spectrum by known mathematical methods.

No conclusive physical evidence has been found, however, to indicate a diphase nature in glass, and the previously mentioned methods for treating the viscosity of glasses as a function of the temperature must be regarded as empirical devices in which more constants are introduced to enable one to come closer to the experimental findings.

The method of Barrer, which was mentioned earlier, attempts to account directly for various possible mechanisms and types of units within the body. In this theory it is assumed that (a) the energy of activation is distributed over a zone of degrees of freedom, and (b) cooperation may be needed between rotations of inter-molecular vibrations before the unit act of flow or diffusion can occur. The Maxwell-Boltzmann statistics are applied and various energies of activation and cooperative factors are introduced. It still appears necessary, however, to find these energies of activation and the cooperative factors from experimental data.

Conclusion. The theory of rate processes gives numerical results close to the experimental values in many problems. This is particularly true when one or more of the parameters are treated as adjustable constants and values are assumed which bring about good agreement. When this procedure fails to bring results, more than one species may be postulated and more adjustable constants introduced to be played around with until agreement with experiment is reached. While useful in the absence of better methods, this procedure is hardly satisfying.

On the other hand, models of systems can be set up and, in the course of interpreting the physical processes taking place, absolute values worked out. The value of the method in such cases would appear to come from having a statistical theory made up in advance, capable of many interpretations and ready to apply.

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THE NEW YORK ACADEMY OF SCIENCES

announces

THE A. CRESSY MORRISON PRIZE CONTEST FOR 1949

The New York Academy of Sciences announces the following prize contest for the coming year:

Two prizes of $200 each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Meeting, December, 1949, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

CONDITIONS GOVERNING THIS CONTEST

(1) *Eligibility.* Authors and co-authors competing for prizes in this contest shall be members in good standing of The New York Academy of Sciences or one of its Affiliated Societies, but non-members may become eligible by joining one of these organizations before the closing date.

(2) *Date.* Papers are to be submitted on or prior to October 15, 1949, to the Executive Director of The New York Academy of Sciences, at The American Museum of Natural History, Central Park West at 79th Street, New York 24, N. Y.

(3) *Papers.* All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached, together with an abstract suitable for press release.

(4) *Awards.* The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) *Publication.* The Academy shall have first option on the publication of all papers submitted in competition, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1949 by The New York Academy of Sciences."

Such a statement, in substance, must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

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SECTION OF BIOLOGY

CONFERENCE ON "THE PLACE OF STATISTICAL METHODS IN BIOLOGICAL AND CHEMICAL EXPERIMENTATION": JANUARY 28 AND 29, 1949

The Section of Biology of the Academy, the Biometric Society, and The New York Metropolitan Chapter of the American Statistical Association held a Conference on "The Place of Statistical Methods in Biological and Chemical Experimentation." Doctor Edwin J. de Beer, The Wellcome Research Laboratories, Tuckahoe, New York, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, January 28

Morning Session. Chairman, Edwin J. de Beer, The Wellcome Research Laboratories, Tuckahoe, New York.

"Introductory Remarks," by the Conference Chairman.

"The Statistical Part of the Scientific Method," by George W. Snedecor, Iowa State College, Ames, Iowa.

"The Function of Designs in Experimentation," by Gertrude Cox, Institute of Statistics, University of North Carolina, Raleigh, North Carolina.

"Some Rapid Approximate Statistical Procedures," by Frank Wilcoxon, American Cyanamid Company, Stamford, Connecticut.

Afternoon Session. Chairman, George W. Snedecor, Iowa State College, Ames, Iowa.

"Statistics in Analytical Chemistry," by W. J. Youden, National Bureau of Standards, Washington, D. C.

"A Large Planned Experiment in Penicillin Production," by K. A. Brownlee, E. R. Squibb & Sons, New Brunswick, N. J.

"Statistics in Nutrition Research," by Robert A. Harte, Arlington Chemical Company, Yonkers, N. Y.

"Some Examples of the Use of Statistics in Pharmacology," by Claude V. Winder, Parke, Davis and Company, Detroit, Michigan.

Saturday, January 29

Morning Session. Chairman, Gertrude Cox, Institute of Statistics, University of North Carolina, Raleigh, North Carolina.

"Design of Biological Assays," by Chester I. Bliss, Connecticut Agricultural Experiment Station, New Haven, Connecticut.

"Statistics in Experimental Immunology," by H. C. Batson, Department of Biologic Products, Army Medical Department Research and Graduate School, Army Medical Center, Washington, D. C.

"Statistics in Microbiological Assay," by Lila F. Knudsen, Food and Drug Administration, Washington, D. C.

"Biologic Assays Based on Quantal Responses," by Lloyd C. Miller, Sterling-Winthrop Research Institute, Rensselaer, N. Y.

"Statistics in Biological Assay—The Graded Response," by Bert J. Vos, Division of Pharmacology, Food and Drug Administration, Washington, D. C.

Afternoon Session. Chairman, Lloyd C. Miller, Sterling-Winthrop Research Institute, Rensselaer, N. Y.

"Statistics in Clinical Research—Some General Principles," by Donald Mainland, Dalhousie University, Halifax, Nova Scotia, Canada.

"Statistics in Clinical Research," by D. D. Reid, London School of Hygiene and Tropical Medicine, London, England.

"What is a Mortality Rate?" by H. M. C. Luykx, New York University, New York, N. Y.

"Statistics and World Population," by Forrest E. Linder, Statistical Office, United Nations, Lake Success, Long Island, N. Y.

NEW MEMBERS

Elected January 27, 1949

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Freedman, Robert H., M.D., Internal Medicine Externe, Montifiore Hospital, New York, N. Y.

Fried, Jacob R., M.D., Radiology. Attending Radiotherapist, Montifiore Hospital, New York, N. Y.

Kahn, Arthur J., B.A., Biology. Research Assistant, New York University, New York, N. Y.

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Masterson, Teresa, Ph.D., Chemistry. Assistant Professor, Brooklyn College, Brooklyn, N. Y.

Maurmeyer, Robert, Ph.D., Chemistry. Assistant Professor, Brooklyn College, Brooklyn, N. Y.

Roth, Chester H., Manufacture of Rayon Yarns. President, Chester H. Roth, Inc., New York, N.Y.

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Northey, Elmore H.
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Weld, Julia T.
Wellnitz, Anna M.
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Yonkman, Fredrick F.
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Editor's Newsnotes

President Charles Pratt announced on January 31, that the Board of Trustees of Pratt Institute has appointed Dr. Florence S. Tabor Dean of the School of Home Economics effective as of today.

Dr. Tabor, who has been Professor of Chemistry in the School of Home Economics since 1943, has served as Acting Dean for the past five months, since the resignation of former Dean Joan M. Rock.

Miss Tabor, a native of Dover Plains, Dutchess County, N. Y., is an alumna of Skidmore College. She received her Master of Science degree from New York University, and her Ph.D. in Biological Chemistry from Pennsylvania State College. After serving as chemistry instructor at Wilson College and at Vassar, she accepted a similar position at Pratt Institute in 1935.

She is a member of Sigma Xi and Iota Sigma Pi honoraries; American Chemical Society; American Association for the Advancement of Science; and the New York Academy of Sciences.

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OF
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Editor
ROY WALDO MINER

Associate Editor
PAUL LENIHAN

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SECTION ON GEOLOGY AND MINERALOGY

GARNET EVIDENCE IN ADIRONDACK PETROGENY

By S. BENEDICT LEVIN[*]

It is probably fair to say that no one investigator has acquainted himself personally with the entire Adirondack province, and certainly not with all aspects of Adirondack petrology. This situation stems not only from the restrictions imposed by the geographic magnitude of the province, the roughness of its terrain, and the great diversity of features which it displays, but also from that selective blindness which, perhaps unavoidably, comes with specialization in scientific endeavor.

Under the circumstances, it is not surprising that, out of the more-or-less specialized investigations of several workers, there have emerged almost as many conflicting theories of Adirondack petrogeny; or that some of the views based largely on one kind of evidence are not in accord with those based on another kind of evidence. Yet, paradoxically, the only solution (for the present at least) is more of that specialized research, for each new batch of correlated data helps to resolve some of the conflict and make the overall picture more clear and coherent.

The need for data on Adirondack minerals. Considerable attention has been given, in the Adirondacks, to field mapping; to theoretical considerations of the physical geochemistry of anorthosite petrogeny; to the structural elements of the igneous rocks; to their bulk chemical composition; to the petrography of igneous and metamorphic facies; to certain of the mineral deposits; and to a few other problems of a special nature. There is, however, a great need for chemical and physical data on the constituent minerals of the igneous rocks, and the compositional variation of these minerals in different igneous and metamorphic facies. This is especially true because of the significant role which igneous and metamorphic differentiation played in the petrogenetic history of Adirondack rock suites, and it is evident from the literature that the solution of a number of very fundamental problems has been delayed or misdirected for lack of data on certain constituent minerals of diagnostic value.

In the category of significant constituents for which data are needed, one should include especially the plagioclase feldspars, the pyroxenes, the amphiboles, and the garnets. The present discussion deals with some of the data derived from a partial study of the garnets associated with the anorthosite-gabbro series, and with the bearing of this evidence on certain aspects of Adirondack petrogeny.

Garnet as a diagnostic mineral in the Adirondacks. The potential usefulness of garnet as a diagnostic mineral in the Adirondack province is based upon its extensive occurrence, its compositional range, its response to chemical and physical environment, and its amenability to rapid physical analysis.

(1) The diagnostic value of Adirondack garnet rests in the first instance on its widespread occurrence, both areally and in the variety of facies in which it is found. Garnet is prominent, and locally abundant, in much of the eastern two thirds of the province. It occurs in varietal and hybrid facies of anorthosite, gabbro, syenite, granite, and mixed Grenville gneisses. It is found in association with primary magmatic minerals and textures as well as in wholly metamorphic settings.

(2) The ability of the large-celled crystal lattice of garnet to tolerate a considerable and two-fold variation in composition enhances its usefulness as a diagnostic mineral. In its 11.5 to 12 Angstrom cubic unit cell are: 24 lattice positions, which may be occupied

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at random by divalent cations having radii of the order of .75 to 1.00 Angstrom unit, including especially Mg, Fe, Mn, and Ca; and 16 positions, which may be occupied by trivalent cations in the radius range of approximately .5 to .7 Angstrom units, including especially Al, Cr, and Fe, and possibly Mn. Of the 16 or more combinations or "molecules" theoretically possible by these variations, five occur prominently in Adirondack garnets. These are the Mg-Al (pyrope), the Fe-Al (almandite), the Mn-Al (spessartite), the Ca-Al (grossularite), and the Ca-Fe^{3+} (andradite) combinations, each occurring in solid solution with the others.

(3) Garnet, in its formation, is sensitive to, and ultimately reflects in its composition, the effective chemical environment at the time and place of its crystallization. It is for this reason that the structural tolerance of the garnet lattice for different ions is of diagnostic significance in relating garnets from different facies of a seriatal consanguineous group or in comparing garnets from genetically unrelated facies. In addition to this chemical response, garnet, in many instances, reflects in its textural habit (size and morphology) a response to the physical environment. This is evidently related to its strong force of crystallization.

(4) Because garnet is especially amenable to relatively rapid and accurate physical analysis, it becomes feasible to analyze large numbers of samples as well as samples available only in very minute amounts. By the measurement of index of refraction, specific gravity, and lattice constant, it is possible to calculate composition in terms of 4 component molecules, and, by the additional colorimetric determination of Mn, the fifth molecule may be determined. The five "molecules" mentioned above account for over 99 per cent of any garnet specimen in the Adirondacks.

Garnet in the anorthosite-gabbro series, southeastern Adirondacks. The southeastern Adirondacks is a particularly favorable area for the study of garnet occurrence, habit, compositional variation, and genesis, because of the abundant development there of garnet in, or associated with, members of the anorthosite-gabbro series. Within this area, the garnet deposits of the Gore Mountain-Thirteenth Lake district are especially instructive genetically, and none more so than the famous Barton deposit on the north slope of Gore Mountain. A study of this, and of many other deposits which are associated with anorthosite-gabbro facies, provides a fairly complete picture of the formation of garnet in different settings and of the influence of variations in such factors as the gabbroic versus anorthositic character of the host facies, the degree of metamorphic intensity, etc. The historical picture thus obtained is summarized in the following paragraphs.

Summary of garnet genesis in anorthosite-gabbro series. The anorthosite and gabbro are gradationally related consanguineous products of magmatic differentiation by the segregation of plagioclase crystals in a common magma. The tightly packed crystal clusters became immobile anorthosite masses, and in these the intergranular circulation of end-stage endogenous solutions was at a minimum. The loosely packed clusters (pre-gabbro) contained more interstitial fluid, exhibited intrusive mobility, and were eventually immobilized by the interstitial crystallization of augite. In these gabbroic masses, the intergranular circulation of end-stage (deuteric) solutions was considerable, and the products of the reactions between these solutions and the primary minerals are very much in evidence. The most prominent of these products was corona garnet, developed on a microscopic scale by metasomatic replacement in the peripheral portions of primary plagioclase crystals. Corona garnet was extensively developed in gabbro, less so in gabbroic-anorthosite, and only very locally developed in anorthosite proper. Common congenetic associates of the corona garnet are hornblende and some biotite. As the final product of the deuteric residual solution, grains of ilmenite-magnetite, with or without spinel, were deposited.

In the subsequent regional and contact metamorphism associated with the rise and intrusion of the magmas of the syenite-granite series, the small bodies of gabbro and anorthosite (and the borders of large bodies) were more or less converted by recrystallization and reconstitution into granoblastic metagabbro and meta-anorthosite. In the least affected portions of these bodies, corona garnet (where originally present) survives, together with the primary ophitic texture and primary minerals. But in the more intensely metamorphosed portions, the originally disseminated corona garnet was condensed, *i.e.*, recrystallized into individuals of progressively more equant and euhedral morphology and larger size, the extremely large sizes being attained only along contacts representing loci of most intense contact metamorphism. Obviously, the localization of poikiloblastic and porphyroblastic garnet was controlled, too, by the prior distribution of corona garnet.

The localization of garnet deposits of anorthosite-gabbro derivation, along (or near) the contacts of syenite against those facies, is confirmed by numerous examples in the southeastern Adirondacks. This is especially true of the most advanced garnet type, that is, the giant, hornblende-rimmed garnets of the Barton deposit, the Humphrey Mountain deposit, and the Crehore deposit. It has been suggested by Buddington (1939) that this localization is due to the more favorable conditions found where rocks of different kinds are in contact, and that all the garnet types involved (including the corona garnet) are post-syenite in age. In my opinion, the localization noted is not incidental to the juxtaposition of different rock types, but is directly and genetically due to the locally intense metamorphic influence of the syenite magma, in consequence of which the recrystallization growth of garnet in the anorthosite-gabbro facies was promoted.

Support for the view here advocated is found in the local occurrences of syenite-gabbro and syenite-anorthosite hybrid facies, in which there occurs xenocrystic garnet having the same habit and composition as its counterpart in the unassimilated gabbro and anorthosite. Petrographic mineralogic traverses disclose completely gradational transitions from normal garnet-free syenite into garnetiferous hybrid facies. It is evident, too, that the assimilative process was selective. Except locally, where some plagioclase crystals survived as xenocrysts, the plagioclase of the affected anorthosite-gabbro facies was largely assimilated and subsequently co-crystallized with the potential feldspar of the syenite magma. But the garnet and the hornblende already formed in the metagabbro or meta-anorthosite, prior to hybridization, were not assimilated. They survive in the hybrid as unresorbed garnet xenocrysts and irregular xenolithic masses of coarse, black hornblende. Because of this selective character of the assimilation, the more hornblendic, less feldspathic (i.e., the gabbroic) facies were less amenable to hybridization than were the less hornblendic, more feldspathic facies (i.e., anorthosite).

Thus, hybridization was restricted to narrow peripheral portions of the gabbroic masses. While small gabbroic bodies wholly enclosed in syenite retained their identity in spite of reconstitution into coarse, hornblendic, garnetiferous metagabbro, some small or thin bodies of anorthosite suffered thorough-going hybridization, and were thereby converted into phantom xenoliths of hybrid character, with indefinite borders, transitional into the syenitic country rock.

In these syenitic hybrids, the amount and habit of the garnet present depend upon the original character of the hybridized facies and the stage of metamorphic garnet growth attained prior to hybridization. On this basis, two types of garnet ore may be distinguished in the deposits of anorthosite-gabbro derivation: (1) *Dark ore,* which consists of garnet porphyroblasts set in a richly hornblendic matrix. This type is simply a coarse metagabbro. (2) *Light ore,* which consists of garnet porphyroblasts in a highly feldspathic matrix. This type may be either a normal meta-anorthosite, originally of somewhat gabbroic character, or a syenitic hybrid facies of the type already described. The occurrence of both types may be illustrated briefly by examples from actual deposits.

The *Barton deposit* is the world's largest producer of abrasive garnet, and is well known for its spectacularly large hornblende-rimmed garnets. The deposit, containing about 11 per cent of garnet, is an elgonate body several hundred feet wide, over a mile long, and essentially coextensive with the intrusive contact between the regional quartz pyroxene syenite (on the south) and a body of gabbro (on the north). The gabbro is part of an anorthosite-gabbro outlier, and smaller xenoliths of anorthosite occur elsewhere in the surrounding syenitic country rock.

The bulk of the deposit is of the dark ore (metagabbro) type, containing large garnets, one to thirty (but mostly three to ten) inches in diameter and individually enclosed in a shell or rim of coarse black hornblende. A minor part of the deposit is the narrow zone of (hybrid) light ore found along the southern edge, *i.e.* on the syenite side. The light ore bears an intrusive relationship to the dark ore, and contains xenolithic masses of the hornblende and a variable percentage of porphyroblastic garnet xenocrysts one to three inches in diameter. The syenite at a short distance from the contact contains no garnet. But, in traversing northward from the deposit into the core of the gabbro body, the following completely gradational sequence is observed: (1) Dark ore, a very coarse granoblastic metagabbro, with large spherical to euhedral, hornblende-rimmed garnets (XH type). (2) A zone of coarse metagabbro with rimless garnet porphyroblasts (X type) up to about one inch diameter and quite equant in shape. (3) A zone of metagabbro with only occasional relics of the primary ophitic texture and mineral assemblage; in the dominant reconstituted portion, an abundance of millimeter-sized, very irregular, poikiloblasts of garnet (P type); in the relic portions of primary character, microscopic corona garnet (C type). (4) The ophitic gabbro, primary in all respects, except for the abundant development (up to 25 per cent) of corona garnet, C type, in the peripheral zone of the plagioclase crystals. The four textural types of garnet correspond closely in composition: all are predominantly pyrope-almandite, with only about 15 to 20 per cent of lime garnet.

As already indicated, the four types C, P, X, XH, are here interpreted as constituting a genetically related series in which the C type (which is associated only with the primary minerals and textures) is the parental type from which the others were sequentially derived by recrystallization under conditions of progressively greater metamorphic intensity. The XH garnets are not different genetically from the other (smaller, rimless) porphyroblasts, except that, in growing to such extraordinary size in a matrix so rich in hornblende, they displaced the hornblende, which accumulated around the growing garnet to form the rim.

The *Humphrey Mountain deposit*, on Humphrey Mountain along the western edge of the Thirteenth Lake quadrangle, is very similar, in setting and features, to the Barton deposit, except that no light ore zone was noted there. The deposit is of dark ore (metagabbro) type, and the garnets reached the XH stage of growth, although none larger than about eight inches in diameter was observed. The *Ruby Mountain* deposits, north of Thirteenth Lake, are examples of metagabbroic xenoliths in syenite. The garnets are porphyroblasts an inch or two in diameter.

The *Hooper deposit*, one mile east of Thirteenth Lake, is a striking example of the light ore type of hybrid facies. The deposit is a foliated feldspathic rock in which the foliation is brought out by streaks of hornblende and spherical, rimless porphyroblasts of garnet about an inch in diameter. But the matrix feldspar is not the microperthite of the surrounding syenite. It is a plagioclase, corresponding to that in some of the light ore hybrid at Gore Mountain. The garnets are pyrope-almandite with 15 to 20 per cent lime garnet. The deposit is interpreted as a syenitic hybrid of gabbroic anorthosite, a phantom xenolith peripherally gradational into the surrounding syenite.

Primary foliated garnetiferous anorthosite, or meta-anorthosite? A considerable part of Balk's (1931–32) structural evidence for his theory of petrogenesis of Adirondack anorthosite and gabbro hinges upon his interpretation of foliated garnetiferous anorthosite as being a primary facies. That is, on finding garnet individuals, a prominent constituent (together with hornblende) of the dark streaks and lenticles which mark the foliation of border facies anorthosite, he regarded such garnet as an early magmatic mineral which had become involved in the development of magmatic flowage foliation.

If the conclusions which I have reached concerning garnet genesis in anorthosite and gabbro are correct, it would seem that much of Balk's structural evidence is in need of re-examination. For, in the foliated meta-anorthosite at Gore Mountain, one finds the same kind of garnet occurrence as that with which Balk dealt. But, in this area, the evidence of relic ophitic portions of the anorthosite, with corona garnet, and the evidence of transitional features, indicate that the garnets are metamorphic porphyroblasts rather than phenocrysts, and that the foliated, granoblastic, garnetiferous (meta) anorthosite is the product of metamorphic recrystallization from originally ophitic, partly protoclastic, gabbroic anorthosite.

Skarn, or gabbro in statu nascendi, or meta-anorthosite? Another fundamental problem, closely related to the one just mentioned, concerns the genetic interpretation of the streaks, schliere, or lenses of dark minerals (chiefly, garnet and hornblende) which are so prominent in the foliated anorthosite near Schroon River, within the southeastern border of the massif. Balk (1931) interpreted these dark schliere as the products of flow segregation of primary magmatic minerals, and regarded them—the dark schliere—as illustrative of gabbros in the initial stage of formation. Buddington (1939) interpreted the same features as shreds of Grenville skarn.

In the hope that garnet data might help to resolve the problem, I visited the same area, examined the host rock, and later analyzed some of the garnet. I found the host rock to be a granoblastic meta-anorthosite similar to that which occurs in the Gore Mountain area, and the garnet to be similar in habit and composition to the P and X type garnet of the Gore Mountain meta-anorthosite. The garnet compositional ratios found were: $Mg_{12}Fe_{53}Mn_1Ca_{34}$ at Gore Mt.; $Mg_8Fe_{53}Mn_1Ca_{38}$ at Schroon River. Since, in skarn garnet, Ca almost invariably exceeds 85 mol per cent, while Mg is either absent or very low, and since these conditions are met by all Adirondack garnet of definite skarn origin reported thus far, it is a fair preliminary inference that the Schroon River mineral is more likely a normal meta-anorthosite garnet than a skarn garnet. Here again, if the interpretation of the garnet evidence is correct, then the earlier explanations should be re-examined.

Garnetiferous gabbro-amphibolite, or meta-gabbro? Consistent with his views of gabbro genesis, Balk referred to the lenses or larger masses of dark minerals in anorthosite and in syenite as gabbro-amphibolites, and he regarded them as flow segregations of primary magmatic minerals. Thus, he interpreted the Crehore garnet deposit, near Casey Mtn. in the southern part of the Newcomb quadrangle, as a primary garnetiferous gabbro-amphibolite in syenite. Where larger gabbroic bodies have amphibolitic border facies and massive ophitic pyroxenic core facies, he regarded the former as primary magmatic and the latter as secondary, the result of recrystallization of original gabbro-amphibolite.

If these views were applied to the Barton deposit and to the gabbro body of which it is a part, it would be necessary to regard the garnet deposit, and the giant garnets, as magmatic. Now the garnets are so large and so superior in morphology, when compared with their mineral associates, that, as magmatic phases, they would surely have been among the earliest to form. Also, since they are at least half again as dense as the densest magma in which they might have grown, they should be gravitationally segregated. But they are not. In fact, the larger the garnets, the more distantly they are spaced from each other.

Moreover, an analytical traverse across a diameter of one of the larger Barton garnets (19 inches) discloses that there is a slight but distinct decrease in Fe/Mg ratio from core toward periphery. This is the opposite of what is to be expected in a zoned magmatic ferromagnesian.

The garnet evidence here is in accord with a host of other data pointing to the garnetiferous amphibolitic facies as the metagabbro and to the ophitic pyroxenic facies as the primary gabbro.

Corona garnet and the ilmenite-magnetite deposits. As a final example of the bearing of garnet evidence on Adirondack petrogenetic problems it is of interest to note the prominent occurrence of garnet in association with the ilmenite-magnetite ores of the Sanford Lake district. In that area, large masses of ilmenite-magnetite occur conformably in gabbro and disconformably in anorthosite. Wherever xenolithic anorthosite or plagioclase crystals occur in the ore, and wherever veinlets or dikelets of ore cut the anorthosite, there is a zone of "reaction" minerals between the ore and the anorthosite or plagioclase. Garnet is the most common constituent of the reaction zone, hornblende the next most common.

The garnet, found locally as the major or sole constituent of zones several inches thick, was tentatively reported (Stephenson, 1945) as having the compositional ratio $Fe_{10}Ca_{90}$, corresponding to skarn garnet. Since such a composition would be quite unusual in that setting, and quite difficult to reconcile with the conclusions reached with respect to the origin of the Fe-Ti ore, I took the occasion to verify the garnet composition by physical analysis of a typical specimen. The results are interesting and, I believe, rather significant; for they indicate a composition (approximately $Mg_{42}Fe_{44}Ca_{14}$) almost identical with that of the microscopic corona garnet of the Gore Mtn. gabbro. It will be recalled that the chemical and petrographic evidence at Gore Mtn. indicate a direct genetic relationship between the widely disseminated micrograins of ilmenite-magnetite and the microscopic corona garnet with which they are associated. The conclusion reached was that late-stage, endogenous (*i.e.* deuteric) solutions, moving through (or trapped in) a multitude of micro-channels in the gabbro, first produced corona garnet by reaction with the plagioclase, and finally precipitated the grains of ilmenite-magnetite. From a quite different array of evidence, Stephenson (1945) concluded that the massive ilmenite-magnetite deposits at Sanford Lake are the products of crystallization from a late-stage deuteric residuum. The correspondence between the Gore Mtn. microcorona garnet and the Sanford Lake garnet zones, in terms of mode of occurrence, mineral associates, and composition, thus tends to provide mutual confirmation for the interpretations reached independently in the two separate areas. On the basis of such evidence, one is probably correct in regarding the micro-disseminated ilmenite-magnetite of Adirondack gabbro as the genetic counterpart of the ilmenite-magnetite which, at Sanford Lake, became concentrated into very large masses. That relationship, which has been assumed by some on an *a priori* basis, although supporting evidence was lacking, is of direct and timely bearing on the genesis of titanium deposits in anorthosite-gabbro.

The examples cited serve to illustrate the useful potentialities of correlated garnet data in helping to resolve certain apparent conflicts in Adirondack petrogeny and in facilitating the correlation of independently developed evidence of various kinds. Obviously, there are inherent dangers in applying to one part of the province, by extrapolation, the interpretations based upon evidence derived from another locality. But, since the same fundamental processes were probably operative throughout the province, differing from one locality to another only in degree, it is obvious, too, that the evidence derived from detailed studies of the essential mineral phases of the igneous and meta-igneous rocks is of prime importance in solving the basic petrogenetic problems of the entire province.

Note—A more detailed and documented presentation of the evidence and arguments bearing on the genesis of some Adirondack garnet deposits has been prepared by the author for publication by the Geological Society of America

DIVISION OF MYCOLOGY[*]

NUCLEAR CYTOLOGY OF FUNGI

By EDWARD D DeLAMATER[†]

The nuclear activity of various fungi has long been observed in the work of R. A. Harper,[1] Dangeard,[2] Juel,[3] and others,[4] who demonstrated beyond the question of a doubt that typical mitosis occurred in all of the fungi observed. That these nuclear activities were comparable to, if not identical with, what occurs in higher organisms with larger cells, could not be questioned. However, because of the size of the nuclei of these organisms, the limits imposed by the microscopical resolution obtainable with white light, and the difficulties encountered because of inadequate definition of nuclear structures with non-specific staining methods, little more could be ascertained. It has not been until the recent work of McClintock[5] on the Neurospora in which she used the aceto-orcein method, and the work of Robinow and DeLamater[7, 8] who developed special techniques in which the ribose nucleic acids in the cytoplasm were removed by acid hydrolysis, that a clear-cut picture of the nuclear activity of these minute nuclei and their contained chromosomes could be observed. The elimination of the reducing substances which occur normally in the cytoplasm and obscure the nuclei when they are stained with such basic dyes as hematoxylon, permits future clarification of the whole problem.

The work of Lindsay Olive[9] and that of DeLamater,[10] presented at the American Mycological Meetings, 1948, both demonstrated that there is an elongation and contraction of the chromosomes of fungi during the nuclear cycle. It seems likely that the so-called elongation and contraction of the chromosomes corresponds to the coiling and uncoiling of the chromosomes described in higher forms. In both studies, photographic evidence was presented which showed what was described as beading of the chromosomal threads. This beading appears to correspond to the euchromatin and heterochromatin banding of higher organisms, although this needs further clarification.

The vegetative nuclear cycle of *Blastomyces dermatitidis* has been described in detail and photographed by DeLamater.[11] There is a rather remarkable correspondence between the observations made on this organism and those made by the same author on the diploid vegetative phase of *Saccharomyces ceravisiae.*[10] However, there are one or two points of difference worthy of note.

There are four relatively easily identifiable chromosomes in the diploid phase of *Saccharomyces*, two paired elongated chromosomes, and two paired round chromosomes in the condensed phase. Immediately following telephase, the nucleus enlarges and the chromosomes elongate (uncoil?) into long, delicate, beaded threads which lie irregularly in the nucleus against the nuclear membrane.

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[*] The Section of Biology held a meeting on February 14, 1948, at which Doctor Morris H Harnly, Department of Biology, New York University, N Y, presented a paper entitled "*A Physiological Interpretation of the Effect of Temperature upon Development*," illustrated with lantern slides When this paper is received it will be published in the TRANSACTIONS.

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In the expanded, enlarged, interphase nucleus, the nucleus itself may obtain a size of about two microns, and the chromosomes may obtain a length of from two to four microns or longer, with diameters of 0.25–0.35 microns. As the nucleus progresses toward the next division, the chromosomes contract (coil?) to one side of the nucleus and become very dense. At about this time, a centriole appears on the nuclear membrane opposite the condensed chromosomal mass.[12] This centriole then divides in two, and each daughter centriole moves or migrates to opposite sides of the nucleus. These centrioles are approximately 0.25 microns. When the centrioles or polar bodies lie at opposite sides of the nucleus, the chromosomes lie transversely between them, forming a typical spindle. They then proceed to divide and migrate to the two poles. At the time of division, the distinct chromosomal masses become hazy and difficult to stain, and, as the chromosomes separate, the mass appears to stretch out and pull apart like a piece of taffy. Except in very exceptional and rare preparations, it is impossible with present methods and present magnifications to define the inherent structure within the separating chromosomes at this stage. It does appear, however, to be a valid interpretation that the chromosomes at this stage undergo a second period of elongation or uncoiling and that they are actively stretched as they pull (are pulled?) apart and proceed to the poles. It is this phase of the division, the anaphase-telephase stage, which has been so accurately and repeatedly described by Guillermond,[13] and it is this phase which Guillermond interpreted until recently as evidence for an amitotic nuclear division in the yeasts. Once the chromosomes have arrived at the poles, they again condense or contract into discrete masses, the centriole or polar body disappears, the nucleus again enlarges, and the chromosomes again expand to repeat the cycle as described.

In *Blastomyces*, which is a multinucleate cell, the new buds appear to form independently of nuclear activity unless their development is related to the nucleus-cytoplasm ratio. The nuclei migrate without division during their interphase, or vegetative phase, into the new bud.[11] In *Saccharomyces*, which is a uninucleate cell, the bud forms apparently independently of the stage of the nuclear division and, during the interphase or early prophase, the axis of the nuclear division may occur in any direction and need not be related, apparently, to the point of origin of the bud. Cases have been observed in which the axis of the nuclear division points directly into the bud; others have been observed in which the axis of the nuclear division occurs in the longitudinal axis of the parent cell; and still others have been observed in which the axis of the nuclear division occurs at any angle to the origin of the bud. It has been interesting to observe that, during the anaphase-telephase stage of the division, the daughter nuclear mass which is proceeding into the bud can become contorted and twisted as it turns corners, *etc.*, to get into the bud.

Definition of what constitutes the nucleus of a yeast cell has come only with the development of new methods. Lindegren,[14] recently considered the nucleus to be the vacuole of the yeast cell, and the object which we here describe as the nucleus to be the centriole. The chromosomes of Lindegren are intra-vacuolar granules, probably consisting of ribosenucleic acid. The granules which Lindegren considers to be chromosomes are eliminated by weak acid hydrolysis[7, 8, 16] and, contrary to the observations of Rafalko,[15] do not stain with the feulgan stain. The only object within the cell which takes the feulgan stain is the body at the side of the cell vacuole, which is here described as the nucleus. It has been possible to confirm in detail with the feulgan stain the observations recorded here, using the aldehyde-mordanted basic fuchsin method, and to confirm the observations of Winge[17] that this is the only feulgan positive body in the cell.

It seems warranted to assume (1) that the nucleus of *Saccharomyces* has now been defined; (2) that chromosomes of a recognizable shape and size have been observed; (3) that the vegetative nuclear cycle conforms to the general pattern characteristic of higher organisms; (4) that a centriole and polar bodies are constantly present in these divisions; (5) that a typical mitotic spindle is produced during the division of the nucleus; and (6) that there are two periods of chromosomal elongation and contraction, or coiling and uncoiling, during the vegetative nuclear cycle.

It is hoped that, in the near future, the complete nuclear cytology of nuclear fusion and mitosis of *Saccharomyces* can be presented with photographs.

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SECTION OF PSYCHOLOGY

PSYCHOLOGICAL PRINCIPLES OF THE VISUAL MOTOR GESTALT TEST

By LAURETTA BENDER[*]

In contrast to association psychology, gestalt psychology states that we do not perceive parts of the stimuli and build them up into images, but that we perceive the whole or total quality of the image. The organization of the stimuli into the image is based upon laws of perception which include proximity, similarity, direction, and inclusiveness of the stimuli. The basic concept is that the whole is more than the sum of its parts. This perceptual experience is called a gestalt or configuration or pattern. Organized units or structuralized configurations are the primary forms of biological reactions. In the sensory field, these gestalten correspond to the configurations of the stimulating world. This is the static concept of the classical gestalt psychologists (Wertheimer, Koffka and Koehler[†]). It fails to account for drives and tendencies of human conduct, growth, retardation in growth, or regression.

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[*] New York University Medical School and the Psychiatric Division of Bellevue Hospital, New York, N. Y. This paper, illustrated with lantern slides, was presented before the meeting of the Section on February 21, 1949.

[†] For references to the literature see Bibliography.[1, 2]

Paul Schilder emphasized the importance of dynamic concepts in gestalt psychology. He stated that there was not only a "Gestalt" but a "Gestaltung," meaning that the organism in the act of perceiving always adds something new to the experienced perception. He said, in the introduction to my monograph, that "the organism does not react to single local stimuli by single responses but by a total process which is the response of the whole organism to the total situation."

The organism has a "gestalt function" which is defined as that function of the integrative organism whereby it responds to a given constellation of stimuli as a whole, the response being a constellation or pattern or gestalt which differs from the original stimulus pattern by the process of the integrative mechanism of the individual who experienced the perception. The whole setting of the stimulus and the whole integrative state of the organism determines the pattern of response.

We find, however, that there is an innate tendency to experience gestalten not only as wholes which are greater than their parts, but in a state of becoming (Eddington), which integrates configurations not only in space but in time. There is a tendency, not only to perceive gestalten, but to complete gestalten and to reorganize them according with principles biologically determined by the sensory motor pattern of action which may be expected to vary in different maturation or growth levels and in pathological states organically or functionally determined.

Even in deviate individuals, *i.e.*, aments or dements, the drive to experience complete gestalten and to contribute to the integration of gestalten are always present. A gestalt is always experienced, but in a more primitive form in which the whole is still greater than all its parts.

The visual motor gestalt test consists of figures (PLATE 1) taken from Wertheimer's classical studies. They are presented one at a time, to be copied by the individual or patient who is being studied.

Figure A is the introductory figure. It consists of a circle and a diamond-oriented square—touching each other on the horizontal plane. This is readily experienced as two closed figures on a background. According to Wertheimer, this configuration is recognized as two contingent figures, because each represents a "gute Gestalt" which refers to a complete, consistent, and coherent configuration or geometrical form.

Figure 1, according to Wertheimer, should be so perceived that the dots appear as a horizontal series of pairs determined by the shortest distance, or with "remnants" left over at each end. This is an example of a gestalt formed on the principle of the proximity of parts.

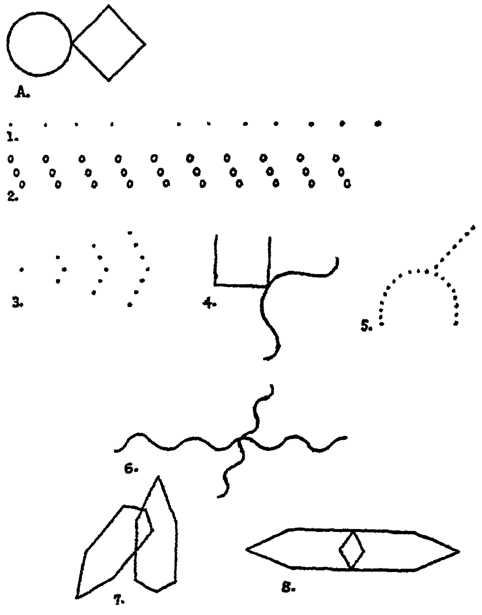

PLATE 1. Test figures adapted from Wertheimer.

Figure 2, according to Wertheimer, is perceived usually as a horizontal series of short, slanting lines consisting of three loops. It is also determined on the principle of proximity of parts. This is also true of Figure 3, formed by dots. Figure 4 is perceived as two units determined by the principle of continuity of geometrical or internal organization: the open square and the bell-shaped form at the lower right hand corner. The same principle holds true for Figure 5, which is seen as an incomplete dotted circle with an upright slanting stroke. Figure 6 is seen as two sinusoidal (wavy) lines crossing each other at a slant. Figures 7 and 8 are two configurations made up of the same units, but they are rarely perceived as such, because in figure 8 the principle of continuity of geometric form prevails—which in this instance are the straight lines at the top and the bottom of the figure.

It is most important to study the genesis of perception in young children, and to follow its maturation. We have long known that children perceive differently from adults. The drawings of *children* from two and one-half to four years are scribblings that represent motor play. These are done for the pleasure of the motor expression, the scribbled pictures being a by-product and having no meaning. They are performed by large arm movements in a dextrad, clockwise whirl, or pendulum waves, if the child uses its right hand; and in sinistrad, counter-clockwise whirls, if the left hand is used. They tend to take on a differentiated form by inhibition into single closed loops or parts of loops. Patterns or gestalten are formed by combinations of these which are adapted to resemble the perceived stimulus or to represent it symbolically. The child finds it difficult to reproduce patterns, but by various motor experimentation, pictures are produced which may finally represent the pattern desired. It is easier for the child to imitate the movements of another person so that scribbling may be limited to single arm movements, to dashes, dots and zigzags. Thus, any dotted form may call forth motor behavior that produces dots, but the tendency may still persist to produce them in loop formation, masses, or series. An enclosed loop is the basis of all perceived form. There is also a tendency to perseverate any one learned (even if self-discovered) pattern wherever adaptable to other perceived figures. Direction, especially dextrad horizontal direction in the right handed child, is more important than distance or size. It is more overwhelming than Wertheimer's principles of proximity or similarity of parts. This predominance of the directional factor is probably due in part to motor features, and in part to the principle that the optic field is organized on movement. Concepts such as series and masses are more readily grasped by children than absolute number or size. Wertheimer's principle of continuity is important to the extent that it involves direction and series. His principles of 'gute Gestalt' and natural geometrical figures are important to the extent that they arise out of the primitive loop. Otherwise, the principles of gestalt, as given by Wertheimer, do not apply to the genesis of gestalt in the maturation of the child's visual motor patterns.

Rapid differentiation of form, maturing of the motor apparatus, and the capacity for object representation take place between the fourth and seventh year.

A. A. Fabian, while he was working with education disability in the problem children at Bellevue and normal children in the public schools, found that the tendency to rotate horizontally directed configuration to the vertical position is found in the normal child of pre-school and beginning-school age. It is a developmental phenomenon which is gradually corrected as the child matures, but does not disappear until he is seven or eight years old. He showed that "muscle mechanics of the arm favor movement in the vertical direction." Movement tendencies in the gestalt function also contribute. Finally, body image factors, described by Schilder as "all the perceptual experiences integrated into a schema which represents one's experiences about one's own body," influence the projected sensory experience of the developing child. Upright posture is a factor also. The close relation with the visual motor gestalt function and language development is emphasized by Fabian. Those children who show reversal tendencies in reading disabilities may be shown in testing with the gestalt test or similar tests to persist in the tendency to verticalization of horizontal figures or to rotation of figures.

Plasticity as a feature of the very young child, which is readily recognized in the gestalt test, especially when it is viewed as a total gestalt on one sheet of paper, was not sufficiently emphasized by me in the earlier work. I have seen it more frequently in schizophrenic children and in some gestalt test material of 149 *native children* from the Chamorros and Carolinians of Saipan in the Mariana Islands, who were examined by Dr. Alice Joseph and Dr. Veronica F. Murray, under the Pacific Science Board of the National Research Council. Drs. Joseph and Murray observed that these children's gestalt drawings differed from the norms for American children, which I had published, and permitted me to study them. I observed that there was a strong drive to accomplish the task well and a high degree of conformity. At the same time, there were many indications of fluidity or plasticity or primitive trends. For example, square corners were rounded. Even so, squares and diamonds were clearly formed. There was a strong tendency to rotate figure or an accentuation of verticalization at all age levels. Dotted lines tended to increase motility and were often expressed as wavy lines. Open figures were closed. In general, there was more movement and action. Also, parts of the figures were influenced by other parts and each figure was influenced by others in the performance and by the frame work of the background. Figures were even tied to each other, and the total test tended to make an organized design. Erasures showed strong tendencies to overcome primitive tendencies for more mature and rigid patterns.

Plate 2 are the gestalt drawings of a *defective schizophrenic* adult. They show that the more primitive sensory motor patterns are dependent on the principle of constant whirling motion. . . . in a vortex, with an associated radiating directional component with a tendency to emphasize the horizontal planes. Fixed points are difficult. There is also a tendency for dots to become loops, for straight lines to curve a little, for parallel lines to radiate toward or away from each other, and for sensory-motor patterns to revert to more primitive principles and to express movement in some way.

Experiments with *optic imagery* in four subjects showed that the inherent quality of all optic images is movement. This movement includes whirling, waving, expansion, contraction, scintillation, molecular movement, *et cetera*. By these movements, all figures are reduced to their simplest form, which was a closed circle with a fixed center but no fixed periphery. A point was a center of a possible circle; a line was a direction which was most typically horizontal dextrad. All configurations actively used some one or more elementary forms of movement in order to realign themselves into a primitive quiescent form.

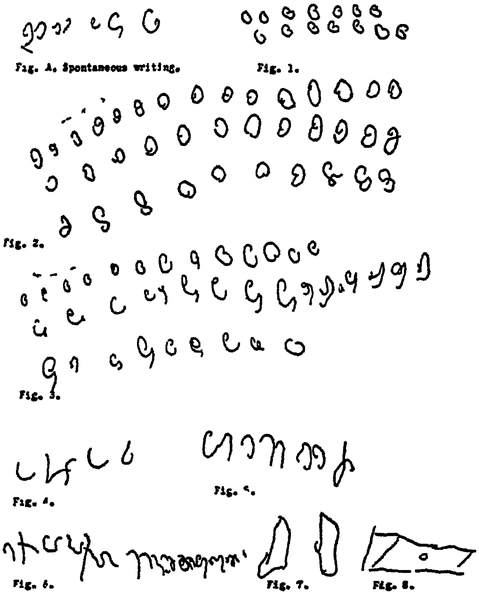
Fig. A. Spontaneous writing.
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.

Plate 2. Gestalt test performed by defective schizophrenic adult.

The use of a short time exposure of the gestalten by the *tachistoscope* lead to the experiencing of more primitive forms. This suggests that the temporal factor is important in total experience and that the active relationship that exists between the observer and the stimulus and which creates the visual motor gestalt requires sufficient time unit for the creation, and that this temporal factor, moreover, results in a different and more mature type of gestalt experience. It seems, therefore, that one important factor in the maturation of visual motor patterns is the integration of the temporal factor into the whole situation and that this is not separable from the total pattern without resulting in a more primitive reaction. The capacity to integrate the temporal factor into immediately created perception requires a certain life span to experience. The concept of maturity rightfully carried with it the idea of ripening with time. Finally, the factors which determine gestalt are:

(1) The stimulating pattern in the physical world, which also must obey its own laws of gestalt.
(2) The motility of the visual field, which determines spatial relationships.
(3) The temporal factor determined by the sequential relationships which tend to become more intricately integrated into the spatial relationships with maturation processes determined by the temporal factor of the life span of the individual.
(4) The motor reaction patterns of the individual, and his attitudes towards and actual participation in the individually created experiences.
(5) The immediate tendency for each of these factors to be nonseparable from the others.

This test was standardized once as a *performance test* for children. Related studies with the gestalt figures convinced me that the visual-motor gestalt function is a fundamental function associated with language ability and closely associated with various functions of intelligence such as visual perception, manual motor ability, memory, temporal and spatial concepts, and organization or representation. Up to the age of four, the small child usually produces scribbles in response to gestalt figures as a stimulus. The Goodenough drawing test of a man starts with a basic mental age of three years. The standardization of gestalt maturation and function covers the ages of four to eleven, which is the age when language function, including reading and writing, are developing. Like the Goodenough, it is not valid after adolescence. The results of attempted standardization are shown in Figure 3 (PLATE 1).

The hypnotic ablation technique of Herbert Spiegel, *et al.*, at the Army Installation Center in the Mason General Hospital, corroborated the results of the standardization on children.

A man with acute transitory *aphasia* due to a thrombosis of a cerebral artery, showed regressive phenomena in motor behavior, verbal speech and visual motor gestalten. Each day he showed the same tendency to approach the problem of copying the gestalt figures in a well organized attempt, which was, however, soon frustrated by an uninhibited tendency to perseverate the units of his own response. The outstanding features in the responses were: The larger principles in the organization of the gestalt were more significant and persistent than the details, and were often exaggerated; there was a tendency to revert to more primitive reactions, such as are seen in children and mental defectives; the responses were more compact, enclosed, and energy-saving; there was an uninhibited perseveration of the unit symbol and of the preceding responses of the principles of the organization, and this tendency was likely to lead to confusion and finally to complete blocking or paralysis; recovery from the cerebral insult showed the same type of progressive integrative maturation, with episodes of sudden insight into the problem as a whole which is shown in normal developing and learning; in this recovery or relearning, the larger principles of the relationships of the gestalt appeared before the details of quality of form and distances of space, or, in Wertheimer's terms, of proximity and similarity.

In general paresis of the insane or *dementia paralytica*, the dull lethargic types, the visual motor gestalten is reproduced with evidence of poverty of impulses and of fragmentation at artificial points in the configuration suggested by the superficial appearance rather than by the genetic gestalt principles. In the expansive types, there is a tendency to substitute letters, numbers, dashes, or the patient's own initial for parts of the configuration, while the whole figure remains fairly intact. This may be akin to confabulation. The whole figure may be disoriented on its background. The general principles of the gestalt are preserved at all times. Some change of a minor nature happens to the concept after it is correctly conceived as a whole. Early cases of *dementia paralytica* in well integrated persons, or cases in which treatment has been administered early, show certain compensatory features. There is a tendency to formalistic but impersonal perfection. Nearly always, however, there is also some telltale feature suggesting one or another type of deviation, or reversion to some primitive feature, such as the use of the closed loop or perseverations in horizontal dextrad directions. These primitive reversions are more common in cases with aphasic, agnostic or apraxic features.

The *alcoholic psychoses* are associated with deep clouding of consciousness, perceptive difficulties resulting in incompletely perceived gestalten, and motor difficulties with increased impulses to rhythmic movements. By this combination, the visual motor gestalten in copied figures are profoundly disturbed, so that the figure is reproduced incompletely and is distorted by perseverated strokes. With less clouding of consciousness, more features of disturbed motor impulses in the perseverative tendencies of the figure are produced. This is a perseveration of motor impulses or rhythmic movement and not a perseveration of form, such as is seen in the sensory aphasias. In chronic alcoholic hallucinatory states, the gestalt as a whole is well perceived, but the outlines are hazy, perhaps partly from perceptive difficulties, partly from tremulousness, and partly from the motor impulse to reiterate the line. In the confusional states, there are less marked motor difficulties, but the perceptive difficulties lead to disturbances in the integration of the parts into the whole and in the orientation of the figure on the background.

The study of the visual-motor gestalt function in *mentally defective children* shows that the problem of mental deficiency is not a simple one. If we were to assume a slow-up or simplification of the maturation process in a unified way, we would expect less differentiation, a more unified system, a stronger and simpler gestalt, such as we find in the younger normal child. This does occur in some individuals, especially among the higher-grade defectives. Even in these cases, motor control is usually better than in the normal children of the younger age. Small, energy conserving figures are the rule. The primitive loop is freely used with less motor play or experimentation. The patterns are more rigid. Individuals who are functioning as mental defectives show evidence in their gestalt drawings of more or less aphasic disturbances which are characterized by the use of a perseverated, primitive, symbolic unit; others show the dissociative phenomena characteristic of schizophrenia; still others show disturbances in impulses with a poverty of responses or hyperkinetic features, such as occur in some types of encephalitis; and, finally, others show perceptive difficulties, confusional features which may be related to focal brain pathology. Such analysis leads us to the opinion that there are multiple causes of mental defectiveness, which might be differentiated with the hope of more specific treatment, training, and prevention.

My first experience with the clinical use of the visual-motor gestalt test was with the *schizophrenic* patients at Springfield State Hospital in 1929 to 1930. (See Plate 2.) Gestalt drawings of typical schizophrenic patients are easily recognized by the bizarreness of the configurations produced, the frequent splitting in figures, not in such a way as to make a "gute Gestalt," and especially by showing an unusual cohesion between all the figures and an increase in movement in the figures. Attempts to use these figures as matrices for delusional ideations by ornamenting them with connecting lines, destroying the original gestalt and creating new figures, are usually noticeable. In such instances, the gestalt test really becomes a projective technique by allowing the person to be tested to project his own ideations into the test performance.

Recently, I have had more experience with *childhood schizophrenics* and have found that all their behavior, including their gestalt functioning, is characterized by a striking plasticity or fluidity due to the tendency to use old primitive responses interlocked with the more mature capacities which are expected from the maturational level of the child. There is, therefore, an excessive use of the vortical movement even with good diamond forms. A series of figures on a horizontal plane may be pulled around into a vortical figure. The boundaries of circles are uncertain and may be gone over several times. The centers of circles are uncertain. There are no points but many little circles. Angular and crossed forms are fragmented. Action cannot be readily controlled and figures are elaborated, enlarged, repeated. The total product makes a pattern itself with a great deal of fluidity based upon vortical movement. The perceptual patterns lose their boundaries and therefore their relationship to the background. There is cohesiveness between the boundaries of two objects. In this well-patterned fluid matrix, there are areas in which the pattern is broken; a part of a figure is separated from the whole and made to rotate faster; a group of small circles is separated from the whole mass. It is as though in a circular stram of rippling water, a pebble was thrown, causing a new wave movement.

In my own studies, I did not expect to find disturbances in perception or in the visual motor gestalt function in the *psychoneuroses*. However, it was often possible to evaluate severe anxiety and compulsive obsessional features, especially when they occurred as defense mechanisms in individuals with organic disorders or psychoses which threatened the integrity of the organism.

During the war, this test was used in a number of Army Installation Centers to aid in differentiating between emotional regression and the effects of organic brain injury or schizophrenia. It was also used in an effort to explore *personality dynamics*, but the results of this research are still inconclusive.

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SECTION OF ANTHROPOLOGY

THE TREE OF CULTURE

By RALPH LINTON*

Current interest in the functional and psychological approaches to culture has made the study of culture origins unfashionable. However, the writer believes that these still have real value. Whatever the functions or psychological connotations of culture traits, their form can be understood only when their origin is taken into account. The present paper is an attempt to present a brief summary of the development and differentiation of cultures in the light of our present knowledge. Much of the information which we would like to have as a basis for this is and always will be lacking, but we have enough to make possible a tentative reconstruction. Even this may be useful on the basis of Bacon's dictum that Truth can be brought out of Error more easily than out of Chaos.

The tree simile for the development and differentiation of living forms is already familiar. However, this is usually pictured as a tree of the temperate zone with a single sturdy trunk from which spring increasingly diversified branches and twigs. The tree of culture bears more resemblance to a banyan tree in which branches cross and fuse, and aerial roots, finding favorable ground, turn to trunks stronger than the parent one. The result is a miniature jungle, but one whose development can be traced step by step.

It appears from our present knowledge that culture is older than our species and may even have played a significant part in fixing certain of its characteristics. By the Middle Pleistocene, three cultures appear to have been in existence: A Large Flake culture extending across most of northern Eurasia, the Biface culture extending over southwestern Eurasia and Africa, and the Pebble Axe culture centering in southeastern Asia and extending westward to Central India and northward for a still undetermined distance. The Large Flake culture seems to have represented an adjustment to a cold environment in which a predominantly hunting economy would have been necessary. Since few of the implements belonging to this culture are themselves suited for use as weapons, one must assume that there was a considerable inventory of objects of perishable material. Whether these three old cultures were the work of different Hominoid species is not important for a reconstruction of culture history. A species able to invent and manufacture tools could hardly fail to copy the tools of another species so closely related that both had essentially the same psychological and physiological equipment.

By the time of the last glacial advance, the Large Flake and Biface cultures had fused in Western Eurasia to produce the Middle Paleolithic cultures, predominantly flake-using and hunting. The Biface cultures seem to have survived longest in Africa. The Pebble Axe cultures, on the other hand, seem to have undergone little modification, possibly reflecting the slight climatic changes in the region in which they existed. The western Paleolithic typologies are quite inapplicable to this region.

Toward the close of the Pleistocene, the Western Eurasiatic cultures show a tendency to employ an increasing number of materials, limiting each to the implements for which it was best suited. Thus bone, antler, and ivory replaced stone at many points. The apparent stagnation of culture in the Pebble Axe tradition may reflect an increasing dependence here on perishable materials, especially bamboo, which would render many types of stone implements unnecessary. Toward the close of the Upper Paleolithic, composite tools based on microliths were developed in Western Eurasia and spread to Africa where they survived, until recent times, in extreme South Africa.

By the close of the Pleistocene, *Homo*

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sapiens had spread to approximately his present limits. Outlying islands had not been reached and the northern limits of occupation fluctuated with climatic changes, but both Australia and the Americas had been occupied. There is evidence of a great number of local cultures at this period, each, presumably, representing an adjustment to a particular ecology. It may be assumed that the world's population was small and that each local group was in fairly stable adjustment to the food supply within its range. Since local cultures were increasingly adjusted to local environments, there was probably little large scale migration, except in areas where climatic change made it desirable. In spite of the relatively small population, there seems to have been great diversity in physical type,understandable on the basis of life in small, inbred communities.

The economy of this period included fishing in favored localities, but dwellers in forest and semi-arid regions were predominantly gatherers of vegetable foods. Hunting economy predominated in northern Eurasia, including the steppes, in the African plateau, and in the Northern American Plains. The stage was thus set for the next step in cultural development, the invention of agriculture, followed by the domestication of animals. Agriculture was, understandably, invented by groups who already depended primarily on vegetable foods. Its early appearance in several semi-arid areas in both the Old and New Worlds stems from the necessity for people living in such regions to depend heavily on vegetable foods, especially seeds.

In the Old World, there appear to have been two principal centers for the domestication of plants and animals. The more important, in Southwest Asia and adjoining sections of Africa, specialized in grains and legumes, *i.e.* seed crops. In this region, the ox, ass, sheep, and goat were also domesticated. This region became ancestral to all the higher civilizations of the Old World, with the possible exception of China. Even here, its contribution was considerable. There appears to have been a second center of domestication in Southeast Asia based upon jungle crops, among which, banana, breadfruit, yam, and taro were most important. Associated animals were the pig and chicken. This Southeast Asiatic center was not responsible for the development of any urban cultures, but strongly influenced Oceania and parts of India and may have exerted some influence northward into southern China. Since its crops were dependent upon tropical conditions, its possible extensions were strictly limited.

China presents a puzzle which cannot be solved until we know much more about the archaeology of the southern part of the country. Russian researches into plant origins assign a large number of domestic plants to this region, but only two of these, soy bean and millet, seem adequate for the support of an agricultural population. The appearance of rice during the Neolithic period in northern China also presents an interesting problem, since this plan seems to have been domesticated in India and is poorly suited to the north Chinese environment.

The development of food raising in various Eurasiatic centers cut off further development of most of the food-gathering and hunting cultures. With the assured food supply which agriculture gave, groups which had adopted it could increase rapidly and take up more and more of the food gatherers' territory. The only regions which were safe from this encroachment were those in which the agriculturalists' crops could not be grown. In Eurasia, this comprised the whole boreal forest zone and northern steppes. Here, a hunting and fishing culture was able to survive, perfecting its adjustment to a difficult environment and, finally, adding to its original equipment of Upper Paleolithic type such appliances as the sled, snow shoe, and ski.

In Africa, the cattle which formed part of the Southwest Asiatic food raising complex could adapt to the conditions of the African plateau and, with the dairying techniques, made possible settlement of this region on a small village basis. However, the crops of the Southwest Asiatic center, even those of its Abyssinian subcenter, could not be raised in Equatorial Africa. The only exception to this seems to have been millet, one variety of which was domesticated in Abyssinia. As a result, the development of higher culture in tropical Africa was delayed until relatively late times. In spite of long contact with Egypt and, less directly, with the Mediterranean hearth of civilization, tropical Africa lagged far behind most of the Old World. This was because it had no food supply adequate to support a large settled population until the introduction of Southeast Asiatic and American food plants.

In spite of these limitations, the basic patterns of village life, which we are accustomed to call Neolithic, spread from Southwestern Asia over most of Europe and North Africa. To the eastward, they spread into India, but there encountered competing influences from the Southeast Asiatic center. While the Southwest Asiatic patterns seem to have established themselves almost *in toto* in the Indus valley and adjoining regions suited to grain raising, they failed to penetrate the eastern and southern parts of the peninsula. Eventually, elements from the two sources fused to produce Hindu culture which owes some of its most striking features, such as the caste system, to the re-working of Southeast Asiatic elements.

In the steppes, the intrusion of Southwest Asiatic village patterns was limited by climatic factors, important among which were the long range fluctuations in rainfall. Here, as in Africa, cattle proved more adaptable than crops and the result was the emergence of a dairying economy eked out by incidental cultivation. It was this economy which was carried westward into Europe by migrants during the later Neolithic and Early Bronze Age. This dairying culture shows many interesting similarities to that developed in the African plateau. It is not to be confused with the full nomadism of later steppe peoples such as the Tartars. This last was a late and special development based upon a fusion of certain elements from the dairying culture and from the boreal forest culture.

The origins of Chinese culture present a puzzle. The earliest Neolithic remains indicate a culture which seems to have many boreal forest elements, plus millet agriculture, and with the dog and pig as the only domestic animals. Toward the close of the Neolithic, a few elements of Western origin, notably painted pottery, appear, while with the emergence of the Bronze Age Shang dynasty there are still more, including the cultivation of wheat, and the whole complex of southwest Asiatic animals except the ass. However, dairying is still lacking and the culture, as a whole, has a strong local character which makes it appear as though the southwest Asiatic elements were due to diffusion rather than actual invasion from the west.

In Southwestern Asia itself, progressive desiccation resulted in the development of still another type of culture. As the areas in which agriculture was possible shrank, the regions between came to be occupied by pastoral nomads who existed in a symbiotic relation with the settled villagers. In sharp distinction to the later nomads of the steppes, these people did not develop a self-sufficient economy. Since they were in contact with settled agriculturalists throughout their history, they adapted to this and became dependent upon the settled people for practically all their material culture and even for the bread which was their principal food.

Turning to the New World, it must be admitted that our information on the earliest settlement of America is still woefully inadequate. The problem turns on two points: what were the first Eurasiatic cultures to reach the Bering Strait region, and what were the climatic conditions in this region at various times? Until these questions have been settled, we cannot get far. There can be very little question that cultures of the boreal forest type were carried across the Strait into the New World, but it remains to be seen whether these were the earliest. Eastern Asia is far outside the known distribution of the easily identified Biface cultures, although a few doubtful Biface implements have been reported from western China. If Lower Paleolithic people reached the neighborhood of the Strait, they presumably had cultures of the Pebble Axe type or possibly some variant of the Large Flake culture. The Northeastern limits of both of these are still unknown. According to Dr. de Terra, the oldest implements from Mexico do show East Asiatic parallels (Dr. He mut de Terra, personal communication), although they show no relation to the well-known Folsom and Yuman types found farther north.

It is becoming increasingly clear that there have been considerable fluctuations of climate in the Bering Strait region during and since the Pleistocene. Not only did a number of Asiatic temperate climate forms, such as the bison, enter America from Asia during the Pleistocene, but, in quite recent times, prairies seem to have extended right up to the Strait on the American side. The not infrequent changes in sea level which took place during the Pleistocene must also be reckoned with, since the closing of the Strait must have meant a much warmer climate to the south of it than we have at present. It seems quite possible, on all grounds, that man entered the American continent at least as early as the last interglacial, but if his culture was of Pebble Axe type, it will be difficult to prove it. The implements of this culture are crude enough to be taken for rejects unless they are found in an unmixed site under test conditions.

What we do know of early North American cultures is that even by Folsom times there were two distinct types recognizable. One of these, represented by Folsom and Yuman, with eastern derivatives, seems to have followed a big game hunting economy. The other, represented by a series of much less spectacular cultures, seems to have subsisted on small game and vegetable foods. This second culture type has been most clearly identified in the southern Rocky Mountain plateau. If these two cultures do represent the earliest types in America, one might anticipate that it would be the small game, vegetable food economy which would be carried southward by migrants. The great game herds, on which the Folsom-Yuman hunters seem to have depended, did not penetrate far into Mexico.

If the dates assigned, even to Folsom, are correct, the settlement of America must have been followed by a long period of something approaching cultural stasis, relieved only by periodic invasions of new peoples coming across the straits. However, as the boreal culture took more and more adequate form in the Old World, there would have been less and less tendency for such migrants to spread southward beyond the limits of the ecology to which their culture was adapted. The next stage in the development of the American cultures was set in train here, as in the Old World, by the rise of food raising.

It has already been said that only people who were already primarily dependent upon wild vegetable foods would be likely to develop agriculture. Since the only really good hunting territory in the New World was the Great Plains of North America and, possibly, parts of the boreal forest, most of the early American settlers fulfilled this requirement. While there seem to have been certain centers for intensive plant domestication, there were also, if the botanists are to be believed, many domestications of single species in particular regions. Certainly, there was a tremendous amount of diffusion of domesticated plants from their points of origin, the wide distribution of maize being a case in point. The American food-raising economy was hampered by a lack of adequate domestic animals and a consequent lack of dairy products. To replace these, a number of high protein crops were domesticated and diffused. The writer believes that a check on the emergent centers of higher civilization in the New World would show that these were correlated with the establishment, as a result of plant diffusion from various directions, of a combination of crops which would provide a balanced ration.

At present, it seems as though the first center in which this was achieved was the Andean region. Extensive developments in technology, art and social inventions followed rapidly, then stabilized and continued with only minor changes until the Spanish conquest. The spread of this culture to the east was inhibited by climatic factors. How far it spread to the north and whether it contributed to the emergence of the high meso-American cultures is still a moot point. However, we know that there was a second center of civilization in Meso-America which influenced both the Southwestern and Southeastern United States. The rise of higher cultures in both these United States centers was exceedingly late and their potentialities were still only partially exploited when the Europeans arrived. In South America, the penetration of the jungle and the establishment of the characteristic jungle technology seems to have been post-agricultural, but the formidable environment prevented development of high cultures.

It is hoped that the foregoing may provide a rough outline for the understanding of the origins and inter-relations of some of the principal lines of culture development. Much has, of necessity, been omitted especially in the treatment of the New World, where conditions are well known to the present audience.

SECTION OF PHYSICS AND CHEMISTRY

A DISSIPATION MECHANISM IN GAS FLOWS AND SIMILAR PHYSICAL SYSTEMS

By PAUL LIEBER*

Introduction

The behavior of systems with many degrees of freedom, which obey the laws of classical mechanics, can be predicted in principle by integrating the equations of motion of the system that are constructed in accordance with Newton's laws. Although the mathematical difficulties become virtually intractable for such a procedure, the procedure, nevertheless, remains valid, in principle, from a physical point of view. This implies that all gas phenomena governed by the classical laws of mechanics, such as the dissipation of mass flow energy of a gas, in principle, are completely determined by Newtonian mechanics. The concept of probability, on which statistical mechanics and the kinetic theory of gases are based, has been used in conjunction with Newtonian mechanics as an expedient for predicting quantitatively the behavior of mechanical systems with a very large number of degrees of freedom. The kinetic theory of gases, which is concerned, in general, with phenomena of gases that are not in a steady state (such as viscosity, heat conduction, diffusion, etc.), leads to the problem of calculating a distribution function that must satisfy the Boltzmann integral-differential equation. In this very general theory, the mechanics is concealed, to a large extent, by the statistical laws. It, therefore, becomes very difficult (if at all possible) to detect or discover by means of the kinetic theory the mechanisms that control specific phenomena of gases that are not in a steady state, and the extent to which they can be explained by considering only a small number of degrees of freedom. The general validity of a Newtonian description of a gas on a molecular scale, discussed above, suggests that mechanisms which control such gas phenomena may exist in systems consisting of a small, as well as of a large number of degrees of freedom. Such systems may, therefore, be completely described, quantitatively, by the Newtonian laws of motion without recourse to the concept of probability. In such cases, the mechanisms are not determined or modified by statistical laws, but are completely determined by purely mechanical considerations

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* Department of Aeronautical Engineering, Brooklyn Polytechnic Institute, Brooklyn, N Y. This paper, illustrated with lantern slides, was presented at the meeting of the Section on February 1, 1949.

A dissipation mechanism,* for transforming mass flow (mechanical) energy into non-mechanical energy of a gas and similar systems, is proposed and investigated here. The procedure is based on a consideration of a small number of degrees of freedom contained within a system consisting of a very large number of degrees of freedom. This mechanism, which is completely determined by mechanical considerations, explains *how* and *why* mass flow energy is dissipated in a gas. It is shown that between the components (molecules) that transfer momentum only by collisions, the finite extent of these components is a necessary condition for the mechanism to exist. In systems where central (position) forces act as well, this condition is no longer necessary, and the dissipation mechanism does not depend upon momentum transfer by collisions. The mechanism is found to be, in general, reversible; that is, it is shown that it can also transform non-mechanical energy into mechanical energy. This reversibility does not detract from its importance, as it still explains the mechanics of energy dissipation. It is proven however, without the use of the concept of probability, that, under special conditions, mechanical energy once dissipated by the mechanism cannot be completely recovered at any subsequent time.

The problem of *how much* mass flow energy is dissipated or recovered by the mechanism, in a system with a large number of degrees of freedom, not in a steady state, is treated by applying the concept of probability directly to expressions for the dissipation energy before and after a set of typical binary collisions. These expressions are obtained by considering the dissipation mechanism for a typical binary collision.

The dissipation mechanism corresponding to ternary and higher order collisions are also investigated. These collisions, however, are not treated statistically, since they are much less probable than binary collisions. Accordingly, the concept of probability is here used axiomatically for a quantitative evaluation of macroscopic phenomena which are determined mechanically and on a molecular scale by the dissipation mechanism.

It is shown that the dissipation mechanism proposed here adds insight directed toward a physical understanding of shock-wave and some boundary layer phenomena on a molecular and macroscopic scale of observation. An outline of a theory based upon this mechanism and developed for calculating compression and expansion waves in steady sub-sonic and super-sonic flows is given. This work was sponsored by the Office of Naval Research, United States Navy.

Theory

This paper presents the investigation of a dissipation mechanism proposed for dissipating mechanical (macroscopic) energy in mechanical systems consisting of a very large number of degrees of freedom which are contained and, in general, arbitrarily distributed, in a rectangular Cartesian space x, y, z. This mechanism provides a physical understanding on a molecular scale of the mechanics of the specific operations between molecules which transform mechanical energy into dissipation energy. The physical understanding which is acquired on a molecular scale, by considering a small number of degrees of freedom of a mechanical system, is then extended to a macroscopic scale. This is done by first considering the mean values of specific functions (determined by the mechanism) of parameters that describe the states of two particles immediately before and after a typical binary collision. Then, the concept of probability is applied axiomatically toward evaluating and interpreting the mean values of these functions. The mean values of the functions of state so obtained are essentially the most probable values of the corresponding macroscopic functions of state of the system. It is, therefore, important to define carefully the macroscopic functions of state that are considered here in terms of the parameters which describe the state of the system on a molecular scale. A number of these definitions will be found equivalent to those used in the kinetic theory of gases. Others have been suggested by, and stem from, the dissipation mechanism. In general, the systems considered here are not in a steady state, and the definition for the mean value of any function of state of a system refers to its instantaneous value. Unless otherwise stated, it is assumed that the particles or molecules of the systems considered here are perfectly elastic, finite, of equal mass, that they transfer momentum only by collision, and that their only form of energy is kinetic. The dissipation mechanism is not inherently restricted to systems with components that transfer momentum only by collision. It also exists in systems consisting of components that transfer momenta by means of central (position) forces. However, the dissipation mechanism, as it pertains to these systems, is not considered here in any detail.

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* This mechanism was initially described by the author in a manuscript submitted to the Journal of Applied Physics in January 1948.

Mass Density. The mass density, ρ, of a system contained in a finite volume element λ enclosing a point x, y, z, is the limit of the quantity $\frac{mn}{\lambda}$, where λ is sufficiently large compared to the dimensions of the particles and their mean free path. This choice is made in order to ensure a virtually homogenous mass distribution within the element. Furthermore, n is the number of particles contained in λ, m is the mass of a single particle, and M is the total mass enclosed by λ.

Mass Flow Velocity. The Mass flow velocity, $\bar{U}$, of a system of n particles contained in a volume element λ is the mean value of the resultant velocities of each of these particles. Strictly speaking, this definition of mass flow velocity pertains to the system contained in volume element λ enclosing the point x, y, z. However, if λ can be taken sufficiently small, then, for all practical purposes, this may be considered as a definition for the mass flow velocity at the point x, y, z, as well. From this definition, it follows that the mass flow velocity in region λ depends only upon the velocity components of the individual particles contained in λ that are parallel to the mass flow velocity.

Mass Flow Momentum. The mass flow momentum, H, of a system contained in volume element λ is the product of the total mass, M, and the mass flow velocity of the system.

Principal Velocity Component of a Particle. The velocity component of a particle contained in a volume element λ which has the direction of the mass flow velocity of the system is called the principal velocity component of the particle. The direction of mass flow is called the *principal direction* of motion of the particles of the system.

Longitudinal Velocity Component of a Particle. A velocity component of a particle contained in a volume element λ which has a direction opposite to the principal direction of motion, is called a longitudinal velocity component of the particle.

Tranverse Velocity Component of a Particle. A velocity component of a particle contained in a volume element λ that is perpendicular to the mass flow of the system is called a transverse velocity component of the particle.

The mechanical energy, E_m, of a system of particles contained in a volume element, is the product of $M/2$ and the mean value of the square of the principal velocity components of the individual particles of the system. It follows from this definition that only the principal velocity components of the particles contribute to the mechanical (mass flow) energy of the system. The kinetic energy determined by the velocity components of the particles in the longitudinal and transverse directions are designated here as E_L, and E_T, respectively. Accordingly, E_L and E_T represent the dissipation energy of the system.

The Dissipation Mechanism. Consider a rectangular Cartesian space x, y, z, which contains a gas consisting of discrete and finite elastic particles of equal mass. These particles are arbitrarily distributed in space with a velocity distribution, such that, the instantaneous mass flow velocity of the system contained in a finite volume element λ has everywhere the direction of the positive x axis. Accordingly, the positive x axis is everywhere the principal direction of motion of the gas particles. This system can be briefly described as a three dimensional, non-steady, gas flow, in which the mass flow has everywhere the direction of the positive x axis.

Consider any two particles of this gas enclosed by a volume element λ, each having transverse and principal velocity components. Only the principal velocity components of each particle contribute to the mass flow energy in region λ. Accordingly, any mechanism which can decrease the kinetic energy of the principal velocity components, and consequently, increase the kinetic energy of the transverse or the longitudinal components, is a mechanism for dissipating mass flow energy in any region λ. It will be shown that any collision between two such particles is a dissipation mechanism, provided the angle θ formed by the line connecting their centers and the principal direction is different from zero, or integral multiples of π and $\frac{\pi}{2}$. Under suitable conditions, it is found that this mechanism can also transform dissipation energy into mass flow energy, and it is therefore reversible in this sense. This, however, does not detract from its significance, since the mechanism still explains the mechanics of energy dissipation on a molecular scale without recourse to a concept of probability.

In the following analysis, it is assumed that the flow just described is two dimensional, and that the state of the gas flow is independent of the space variable z. This is done for mathematical expediency, since the salient properties of the dissipation mechanism can still be retained, and mathematically described by the analysis.

Analysis. At a given time t_0, consider a collision between any two particles, 1 and 2, of a two dimensional gas flow contained in a volume element λ. Let u_i and v_i be the velocity components, of particle i before collision, which are parallel to the x and y axis respectively. Let u_i' and v_i' be the corresponding velocity components of particle i after collision. The index i assumes the value of 1 and 2. Using the principles for the conservation of Energy and Momentum, the following equations are obtained.

$$v_1' = -u_1 \cos\varphi \sin\varphi + v_1 \sin^2\varphi + u_2 \sin\varphi \cos\varphi + v_2 \cos^2\varphi \tag{1}$$

$$v_2' = -u_2 \cos\varphi \sin\varphi + v_2 \sin^2\varphi + u_1 \sin\varphi \cos\varphi + v_1 \cos^2\varphi$$

$$u_1' = u_2 \sin^2\varphi + u_1 \cos^2\varphi + \cos\varphi \sin\varphi\,(v_2 - v_1) \tag{2}$$

$$u_2' = u_1 \sin^2\varphi + u_2 \cos^2\varphi + \cos\varphi \sin\varphi\,(v_1 - v_2)$$

The angle φ is formed (FIGURE 1) by the perpendicular to the line connecting the centers of the particles and the (principal direction) positive x axis.

It is clear that $v_1' = v_2' = 0$ is a necessary condition that, after a binary collision, all the energy of the particles be in the form of mechanical energy.

Setting $v_1' = v_2' = 0$ in EQUATION 1 and then adding, shows that $v_1 = -v_2$ is a necessary condition for complete recovery of mechanical energy. This result will later be used to prove an interesting theorem. Another condition which is necessary for complete recovery is that both u_1' and u_2' be positive.

The transverse energy E_T', after a binary collision between particles 1 and 2, is

$$\begin{aligned} E_T' &= \tfrac{1}{2}m(v_1'^2 + v_2'^2) \\ &= \tfrac{1}{2}m\{(u_1 - u_2)^2 + 2v_1v_2\}\frac{\sin^2 2\varphi}{2} \\ &\quad + m\{-u_1v_1 - u_2v_2 + v_1u_2 + u_1v_2\} \\ &\quad \sin^2\varphi \sin 2\varphi + \tfrac{1}{2}m\{-u_1v_2 - u_2v_1 \\ &\quad + u_2v_2 + u_1v_1\}\sin 2\varphi \\ &\quad + \tfrac{1}{2}m(v_1^2 + v_2^2)\left(1 - \frac{\sin^2 2\varphi}{2}\right) \end{aligned} \tag{3}$$

EQUATION 3 relates the transverse energies before and after a collision. It is seen from this equation that a term which can contribute significantly to an increase in the transverse energy, and consequently decrease the mechanical energy after collision, is

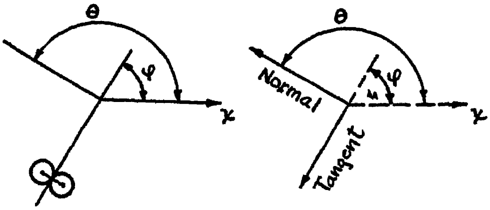

FIGURE 1

$$\tfrac{1}{2}m(u_1 - u_2)^2 \frac{\sin^2 2\varphi}{2}.$$

When the subscripts P or L are attached to a velocity component u_i, they indicate that it is a principal or a longitudinal component respectively. Accordingly, the general expressions for the mechanical and longitudinal energies of two particles after collision are

$$E_M' = \tfrac{1}{2}m(u_{P1}'^2 + u_{P2}'^2) \text{ and } E_L' = \tfrac{1}{2}m(u_{L1}'^2 + u_{L2}'^2)$$

respectively. Whether a velocity component u_i' is principal or longitudinal is determined by EQUATION 2. It is noted that when u_P is finite then u_{Li} must be zero.

The following conclusions follow from EQUATIONS 1, 2, and 3.

(a) A binary collision is a dissipation mechanism, provided that the collision angle φ is different from zero or integral multiples of π and $\frac{\pi}{2}$. This mechanism is purely mechanical in the sense that it is completely determined by Newtonian mechanics without recourse to a probability concept. This is a natural result of the fact that it is determined by a few degrees of freedom.

(b) If u_1 and u_2 are both positive, then a decrease of mechanical energy implies a corresponding increase of transverse energy. That is, under these conditions mechanical energy cannot be directly transferred to longitudinal energy. Conversely, if u_1 and u_2 are positive, then an increase in transverse energy implies an equivalent decrease in mechanical energy.

(c) If u_1 and u_2 are both negative, then longitudinal energy cannot be directly transferred to mechanical energy by collisions. Items (b) and (c) follow directly from EQUATION 2.

(d) If u_1 and u_2 are opposite in sign, then a direct transfer between longitudinal and mechanical energy is possible only under special conditions.

The concepts used to determine, on a molecular scale, the dissipation mechanism of the mechanical systems considered above, also apply to physical systems consisting of components subjected to position (central) forces. In such cases, however, collisions are no longer necessary for the mechanism to exist. The mechanism described here is also inherent in ternary and higher order collisions. This follows, since these higher order collisions consist of one or more sets of binary collisions, to each of which the above analysis is applicable. The mechanism is also inherent in systems consisting of finite particles with unequal masses.

Macroscopic Functions of State-Energy Dissipation by Many Degrees of Freedom

In order to calculate the amount of energy dissipated or recovered by the mechanism in a non-steady gas, it becomes necessary to use a concept of probability. Thus, by using EQUATIONS 1, 2, and 3, the mean values of the velocity components, dissipation energy, and the mechanical energy of two sets of particles that had been engaged in simultaneous binary collisions can be calculated. The mean values of these functions are considered here, since the mean value of any macroscopic function of state is the most probable value of the function. Upon considering many particles that are simultaneously engaged in *binary* collisions, we can calculate the mean value of the transverse dissipation energy resulting from these collisions. Using EQUATION 3, we obtain

$$\tfrac{1}{2}\rho\lambda\overline{(v_1'^2 + v_2'^2)} = \tfrac{1}{2}\rho\lambda\overline{(u_1 - u_2)^2}\,\overline{\frac{\sin^2 2\varphi}{2}} + \tfrac{1}{2}\rho\lambda\overline{(v_1^2 + v_2^2)} - \tfrac{1}{2}\rho\lambda\overline{(v_1^2 + v_2^2)} \cdot \overline{\frac{\sin^2 2\varphi}{2}} + \tfrac{1}{2}\rho\lambda\overline{(v_1^2 + v_2^2)} \qquad (4)$$

non colliding particles. The expressions for the mean values of the velocity components u_1' and u_2' are:

$$\begin{aligned} \overline{u_1^1} &= \overline{u_2}\,\overline{\sin^2 \varphi} + \overline{u_1}\,\overline{\cos^2 \varphi} \\ \overline{u_2^1} &= \overline{u_1}\,\overline{\sin^2 \varphi} + \overline{u_2}\,\overline{\cos^2 \varphi} \end{aligned} \qquad (5)$$

When a bar is placed over a quantity it indicates the mean value of the quantity. In calculating (4) and (5) from EQUATIONS 2 and 3, it is assumed that the statistical distribution of the transverse velocity components is completely symmetrical and that φ is statistically independent of all the velocity components. EQUATIONS 4 and 5 can be used for calculating the effect of simultaneous binary collisions, between two sets of particles 1 and 2 contained in a region λ, upon the change of mechanical energy in the region, with time. The time measure used here is the interval between successive sets of simultaneous collisions.

Using EQUATIONS 4 and 5 it is possible to deduce the following conclusions:

(e) If a system with a sufficiently large mass flow velocity $\bar{u}_1 > 0$, interacts through a set of simultaneous binary collisions with a similar system having a mass flow velocity $\bar{u}_2 > 0$ where $\bar{u}_2 \ll \bar{u}_1$, then the mechanism transforms some mass flow energy into transverse energy. The decrease in mass flow energy is proportional to the quantity $\overline{(u_1 - u_2)^2}$. This clearly shows the relation between compressibility and dissipation that is characteristic of compression shock waves.

(f) In general, there exists a value for the quantity $\overline{(u_1 - u_2)^2}$ causing an increase of dissipation energy, and a decrease of mass flow energy sufficiently large, so that subsequent interaction between systems 1 and 2 causes an *increase* in mass flow energy.

(g) Item (f) implies the interesting result that under suitable and not very stringent conditions, the probability for an increase in mechanical energy (statistical asymmetry) is a ***maximum***. Such phenomena have actually been observed in high speed gas flows; *i.e.*, expansion waves following compression shock waves.[2]

The Intrinsic Mechanical Irreversibility of The Dissipation Mechanism in Some Physical Systems

Consider a gas in which at some time, $t =, t_0$ all the energy of the system is mechanical energy; that is, the resultant velocity of all the particles is in the principal direction of motion. A collision between any of these particles will dissipate some of the mechanical energy, and thereby impart to them a transverse velocity component. Since it was shown earlier that a necessary condition for complete recovery of mechanical energy of two such particles is $v_1 = -v_2$, it follows that at no subsequent time is it possible for the gas to recover by collisions all of its mechanical energy. The proof to this theorem is obtained by showing that some particles of the system which have transverse components cannot satisfy the condition necessary for a complete recovery. Accordingly, it can be proven, *on purely mechanical grounds*, that in some physical systems the dissipation mechanism is irreversible on a macroscopic scale.

Shock Waves and Boundary Layers in Gas Flows

Both compression shock waves and boundary layers are phenomena of gases which involve a transformation of mass flow energy into dissipation energy, and are consequently determined by the dissipation mechanism. The progressive development of a shock wave and a boundary layer imply a progressive reduction of mass flow energy. The EQUATION 3 clearly shows that strong compression is a necessary condition for reducing mass flow energy in a gas flow that is not enclosed by a rough boundary. It also shows, however, that it is possible, under suitable conditions, to increase the mass flow energy of the system. This is confirmed by experiments conducted by Ackeret, Feldmann, and Rott,[2] in which strong expansion waves following compression waves had been observed. A theory based upon the dissipation mechanism described here has been developed and used for calculating steady, one dimensional, compressible flows with compression and expansion waves.[1] The results show good qualitative and, in some cases, quantitative agreement with the experiments, referred to previously.

When a gas flow is confined by a rough boundary, then mass flow energy can be dissipated (in the sense considered here) at the boundary by collisions between individual molecules and the rough elements of the boundary which protrude into the gas. Consequently, in such a case, the compressibility of the gas is no longer necessary for increasing the dissipation energy. This picture explains the progressive growth of dissipation energy in a boundary layer as the flow continues along a rough surface. At this time, there is some indication that the dissipation mechanism may be helpful toward understanding and predicting turbulence.

Conclusions

(1) A mechanism for dissipating and recovering mechanical energy in gas flows and similar physical systems is proposed here. It is shown that this mechanism is inherent in a small number of degrees of freedom of such a system and is therefore completely determined by purely mechanical (non-statistical) considerations. It is shown that the mechanism is in general reversible on a molecular scale.

(2) It is shown that in some systems the mechanism is irreversible on a macroscopic scale.

(3) The dissipation mechanism is applied to systems consisting of many degrees of freedom by means of statistical consideration. In so doing, expressions for the mean values of the mechanical and dissipation energies are calculated. These expressions provide a simple explanation for the progressive growth of a shock wave. They also show that under suitable conditions the mechanism can transform dissipation energy into mechanical energy on a macroscopic scale.

(4) The results obtained here pertain to gas flows which have everywhere the same direction. However, the properties of the dissipation mechanism so determined, can be applied to any small region in mass flows which are not thus restricted.[*]

REFERENCES

1. LIEBER, P. 1949. A statistical theory of one-dimensional gas flows with compression and expansion waves. PIBAL Tech. Rep. No. 136.
2. ACKERET, J., F. FELDMANN & N. ROTT. 1947. Investigation of compression shocks and boundary layer in gases moving at high speed. NACA TM No. 1113.

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[*] The author is very grateful to Dr R P Harrington for his encouragement and helpful criticism The author also wishes to thank Dr Turner Alfrey and Dr Morris Morduchow for their constructive criticism.

CONFERENCE HELD

SECTION OF BIOLOGY

CONFERENCE ON "DEVELOPMENT AND USES OF ANTIMETABOLITES": FEBRUARY 11 AND 12, 1949

The Section of Biology held a Conference on "Development and Uses of Antimetabolites." Doctor D. W. Woolley, The Rockefeller Institute for Medical Research, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, February 11

Morning Session. "Introductory Remarks by the Conference Chairman," D. W. Woolley.

"Biochemical Significance of the Competition between p-Aminobenzoic Acid and the Sulfonamides," by D. D. Woods, Oxford University, England.

"The Utilization of Antimetabolites in the Study of Biochemical Processes in Living Organisms," by W. Shive, University of Texas, Austin, Texas.

Afternoon Session. Chairman, D. D. Woods.

"Investigations on the Selectivity of Action of Antimetabolites," by D. W. Woolley, The Rockefeller Institute for Medical Research, New York, N. Y.

"Ion Antagonism as Related to Antimetabolites," by R. A. MacLeod and E. E. Snell, University of Wisconsin, Madison, Wisconsin.

"Competition between Structurally Similar Steroid Hormones and the Interference with Hormonal Effects by Antivitamins," by R. Hertz and W. W. Tullner, National Cancer Institute, Bethesda, Maryland.

Saturday, February 12

Morning Session. Chairman, W. Shive.

"The Structural Bases of Some Amino Acid Antagonists and Their Microbiological Properties," by K. Dittmer, University of Colorado, Boulder, Colorado.

"Desoxypyridoxine—Observations in 'Acute Pyridoxine Deficiency'," by H. C. Stoerk, Merck Institute for Therapeutic Research, Rahway, N. J.

"Studies on Analogs of Purines and Pyrimidines," by G. H. Hitchings, G. B. Elion, E. A. Falco, and P. B. Russell, The Wellcome Research Laboratories, Tuckahoe, N. Y.

Afternoon Session. Chairman, E. E. Snell.

"Analogs of Pteroylglutamic Acid," by T. H. Jukes, E. L. R. Stokstad, and A. L. Franklin, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

"Folic Acid Analogs in Lower Animals," by E. D. Goldsmith, M. H. Harnly, R. F. Nigrelli, and S. S. Schreiber, New York University, and New York Aquarium, New York Zoological Society, New York, N. Y.

"Studies of the Action of 4-Aminopteroylglutamic Acid and Its Congeners in Mammals," by F. S. Philips, J. B. Thiersch, and F. C. Ferguson, Sloan-Kettering Institute and Cornell University Medical College, New York, N. Y.

"Folic Acid Antagonists and Experimental Tumors," by C. C. Stock, J. H. Burchenal, J. Biesele, D. Karnofsky, A. E. Moore, and K. Sugiura, Sloan-Kettering Institute, New York, N. Y.

NEW MEMBERS

Elected February 24, 1949

LIFE MEMBERSHIP

Falk, Henry C., M.D., Operative Gynecology. Clinical Professor of Gynecology, New York University Post Graduate School, New York, N. Y.

SUSTAINING MEMBERSHIP

Fremont, Rudolph E., M.D., Internal Medicine. Chief, Cardiovascular Section, Halloran V. A. Hospital, Staten Island, N. Y.

ACTIVE MEMBERSHIP

Argyris, Thomas S., A.B , Physiology, Chemistry. Graduate Assistant, Johns Hopkins University, Baltimore, Md.

Austin, J. Harold, M.D., Biochemistry. Professor of Research Medicine, University of Pennsylvania, Philadelphia, Pa.

Bennett, Mary A., Ph.D., Biochemistry. Research Biochemist, Lankenau Hospital Research Institute, Philadelphia, Pa.

Berardi, Gloria Anne, B.S., Endocrinology, Histology, Physiology. Teaching Fellow, New York University, New York, N. Y.

Brandt, J. Leonard, M.D., Cardiovascular-Renal Physiology. Fellow, Department of Medicine, College of Physicians & Surgeons, New York, N. Y.

Brewer, Nathan R., D.V.M., Ph.D., Veterinary Medicine, Physiology. Lecturer in Physiology and Superintendent of Animal Quarters, University of Chicago, Chicago, Illinois.

Brick, Irving B., M.D., Internal Medicine. Chief, Gastroenterology Clinic, Georgetown University Hospital, Washington, D. C.

Burman, Louis R., D.D.S., Periodontia, Anatomy. Assistant Professor in Periodontia, New York University College of Dentistry, New York, N. Y.

Campbell, Delwin M., D.V.S., Veterinary Medicine, Biology. Editor, Veterinary Medicine Magazine, Chicago, Ill.

Carrigan, Paul T., M.D., Medicine, Biology. Resident in Pathology, New York Medical College, New York, N. Y.

Chalmers, Thomas C., M.D., Internal Medicine. Assistant in Medicine, Harvard Medical School, Boston, Mass.

Cohlan, Sidney Q., M.D., Pediatrics. Pediatric Staff, Beth Israel Hospital, New York, N. Y.

Conroy, Edward A., Ph.D , Organic Chemistry. Research, American Cyanamid Company, Bound Brook, N. J.

Cooperman, Jack, Ph.D., Nutrition. Senior Research Biochemist, Hoffmann-La Roche, Nutley, N. J.

Cox, Robert S., Jr., M.S., Biochemistry. DuPont Fellow in Chemistry, Stanford University, Stanford, California.

Crotty, William J., B.S., Biochemistry. Teaching Fellow, New York University, New York, N. Y.

Daniels, Anna K., M.D., Psychotherapy. Private Practice in Psychotherapy and Marriage Counselling, New York, N. Y.

DePuy, John R., B.S , Anthropology, Field Biology. Teacher, Department of Education, American Museum of Natural History, New York, N Y.

Eichna, Ludwig W., M.D., Cardiovascular Research. Associate Professor of Medicine, New York University College of Medicine, New York, N. Y.

Elson, David, A.B., Biochemistry. Student, Columbia University, New York, N. Y.

Evans, Virginia J., Sc.D., Biology, Biochemistry. Cytologist, National Cancer Institute, Bethesda, Maryland

Flax, Martin H., A.M., Biology. Assistant in Zoology, Columbia University, New York, N. Y.

Fontanella, Marion A., M.A., Cytology, Genetics. Anatomy. Instructor, Fordham University, New York, N. Y.

Fromer, Stephen, M.D., Medicine. Physician, Merck & Company, Rahway, N. J.

Garson, Warfield, M.D., Microbiology. In Charge, Antibiotic Research, V.D. Research Laboratory, U. S. Marine Hospital, Staten Island, N. Y.

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EDITOR'S NEWSNOTES

John B. Calkin, New York, has been appointed Director of the Department of Industrial Cooperation, and Associate Professor of Chemical Engineering at the University of Maine, according to a recent announcement by Dr. Arthur A. Hauck, president of the University.

Professor Lyle Jenness, who has been Acting Director of the Department of Industrial Cooperation, will devote his full time to his duties as head of the Department of Chemical Engineering.

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TRANSACTIONS
OF
THE NEW YORK ACADEMY OF SCIENCES

Editor
ROY WALDO MINER

Associate Editor
PAUL LENIHAN

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Executive Director: EUNICE THOMAS MINER

THE BLISTER HYPOTHESIS AND THE OROGENIC CYCLE

By C. W. WOLFE[*]

Introduction. One of the outstanding problems confronting geologists today is the origin of continents and ocean basins, phenomena which are too frequently taken for granted. It is unreasonable to suppose that an originally liquid earth would have been divided on the surface into sialic and simatic sectors. The Pacific basin, in particular, is apparently unique in character, and the obvious conclusion is that this basin, at least, passed through a peculiar and probably cataclysmic stage during which the originally ubiquitous sialic layer was ejected from the region.

This idea has long been known. George Darwin suggested that, since the moon's tidal effect produces a retardation of the earth's rotational velocity, which is accompanied by a retreat of the moon from the earth in order to maintain the angular momentum of the rotating earth-moon system, a reversal in time brings the moon ever nearer the earth, until, at last, it is a part of the earth, protruding from that body as a pronounced bulge. If we imagine that the earth passed through a liquid phase, during which density stratification of the entire planet occurred, we can assume that beneath a surface layer, perhaps 30 km. thick, of the composition of granite an equally thick layer was formed of the composition of diorite which, in turn, was subtended by a layer of the general composition of gabbro to peridotite, probably extending to a depth of at least 600 km. To produce a body of the mass of the moon from the approximate present area of the Pacific basin would require removal of materials from the earth to a depth of about 132 km. The specific gravity of the moon is about 3.45, and this density is consonant with the Pacific origin to the depth already postulated.

It is likely that before the separation of the moon from the earth a universal ocean covered the thin crust then in existence. The ejection of the moon created the ocean basin in the Pacific area, and, for the first time, dry land appeared, erosion began, and sedimentation was possible. Thus, the Archean sedimentation began. During the Paleozoic era, the moon was still relatively close to the earth, and, since the height of tides is, approximately, inversely proportional to the distance of the moon, at that time they were perhaps ten to twenty times their modern height. It was probably this close proximity of the moon which was most responsible for the development of the well-known, widespread, homogenous, epicontinental marine deposits of the Paleozoic era.

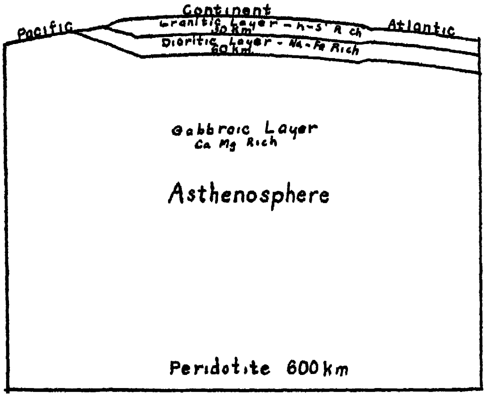

FIGURE 1. Density stratification of outer earth

It is important to realize that this suggestion for the origin of the Pacific basin is not germane to the blister hypothesis that follows. It is given here simply because, while the Blister Hypothesis can adequately explain other ocean basins and also the continents, only some cataclysmic act, such as the moon ejection, can explain the Pacific basin with its magnesium-calcium rich simatic floor. FIGURE 1 indicates the author's view of the distribution of lithologic types in a representative section of the earth today.

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[*] Department of Geology, Boston University, Boston, Mass. This paper, illustrated with lantern slides, was presented at the meeting of the Section on March 7, 1949.

A second problem confronting geologists is the origin of large crustal movements, some essentially vertical, some essentially tangential. This is the problem of plateau formation, of mountain building, and of sinking lands. Although no relation between the origins of these various phenomena need exist, their general association in time and space seems to point strongly toward a common origin for all crustal movements, each perhaps representing a momentary picture in an overall cycle of development. That this might well have been the case will be demonstrated in the following treatment of the Blister Hypothesis.

No adequate theory of earth movements is possible without a clear understanding of the various types of mountains: the erosional, the domal, the fault block, and the compressional mountains. The explanation is no more difficult for one type than for another, although the compressional mountains have received the greatest share of attention from geologists in general. Probably, this is because of the tremendous circumferential shortening which is suggested for such regions as the Alps and the Himalayas. The author believes this shortening to be much exaggerated, and an explanation for the apparent shortening of the crust is readily to be found.

Coupled with the problem of mountain origins is the problem of the constant association of post-Cambrian regional metamorphism and ore emplacement with zones of compressional mountains. These problems move hand-in-hand with the problems concerning the distinctive character of pre-, syn-, and post-tectonic igneous intrusions. Since these, too, in post-Cambrian time, are associated with zones of compressional mountains, it seems entirely reasonable that all these phenomena are interrelated and dependent on the same major cause.

The fact that regions of folded mountains are restricted to definite belts with definite patterns is important to the problem, as is, also, the fact that mountain building is not a continuous process in one region, but is interrupted by what are, apparently, much longer periods of quiescence. In certain sections, however, such as California, quiescence is a relatively rare phenomenon; and this, too, needs explanation.

It is impossible to treat here the objections which have made unacceptable the other hypotheses of mountain building. The fact that no single present hypothesis is accepted by even a sizable minority of geologists is sufficient excuse to warrant the development of another. Perhaps the Blister Hypothesis, as it is presented here, will prove even less acceptable. But even by the rejection of impossibilities progress is made, and thus the hypothesis is here given.

Formation of Blister. A Blister, by definition, is an expanded subcrustal sector of the outer shell of the earth. It is assumed, here, that this expanded region lies in a zone not less than 15 km. and not more than 600 km. below the surface. The physical state of this expanded sector may be either expanded crystalline, glassy, expanded glassy, or, in the final stages, liquid. It is supposed that a Blister may pass through all of these stages before its final collapse and disappearance, but there may be periodic reversals in the trend of development due to the escape of heat, usually in the form of magma.

The cause of the expansion in the Blister is the release of energy associated with radioactivity. Unfortunately for the hypothesis, the amount of radioactivity in depth is not known, although there appears to be enough to make the entire earth liquid if continued through to the center in the same degree of concentration as at the surface. The author believes that the ease with which the Blister Hypothesis explains the various, previously mentioned problems is evidence that radioactivity, to a depth of 600 km., is adequate for the requirements of the hypothesis, since no other adequate energy source can be envisioned.

It is assumed that radioactive elements are not homogeneously disseminated throughout this region. In fact, it could hardly be expected. Thus, it can be imagined that, in certain relatively localized areas, pockets of trapped energy would develop where the loss of heat by conduction and radiation is not so great as the heat produced by radioactive decay. Some heat is, of course, being conducted away at all times. But, if heat gain exceeds heat loss, the rock in that area must necessarily become warmer, expand, and form the nucleus of a Blister. The rate of heat loss would depend primarily upon the physical nature of the rocks wherein the heat is being generated. In surface rocks, with their high crystallinity, their fractures, and their connate and meteoric water content, heat loss exceeds, or is commensurate with, the heat gained from conduction from below, and from radioactivity in the zone itself. But, below a depth of, approximately, 15–20 km., which is here called the *isothermic level*, the physical state of the rocks is such that the rate of heat generation, though probably about the same as in the rocks above the isothermic level, exceeds the rate of conduction away. Thus, the rocks become progressively warmer. The first stage in the development of a Blister, then, is the development of *pocket blisters* in random positions beneath the isothermic level, but above the 600 km. depth. Some would develop in the crystalline, strong crust, and others would develop down in the weak asthenosphere (FIGURE 2).

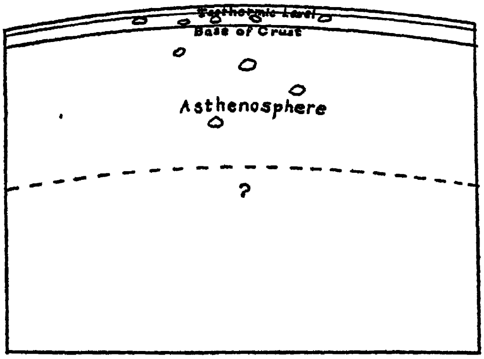

FIGURE 2. Pocket blister phase

Gradually, the Pocket Blister volumes would increase, their boundaries would extend, and coalescence would take place. This stage is known as the *Master Blister* phase (FIGURE 3). It is important to realize that the continued growth of the Blister can be only lateral or in depth, for, above the isothermic level, heat loss will always be commensurate with heat gain, and there will be no pronounced expansion through a change in the physical state of its materials.

It is probable that the proportions of the Blister would approximate a thick, doubly convex, circular lens. The diameter of the lens parallel to the earth's surface would probably be at least 500 km. and might readily be thousands of kilometers, perhaps of continental or oceanic proportions. The vertical thickness would almost certainly reach 100 km. and might easily be more. If we place a conservative estimate of 10 per cent upon the cubic coefficient expansion of rock from the crystalline to the less ordered states, it is immediately apparent that a Blister with a thickness of 100 km. would be manifested at the surface of the earth by a rise of 10 km., which is well within known figures of vertical movements of the crust. An even larger figure is certainly possible. Thus, considerable epeirogenic uplift can be readily explained.

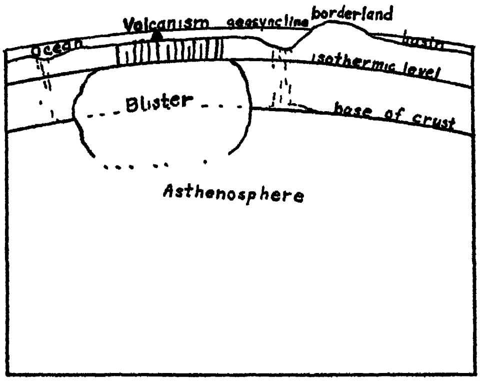

FIGURE 3. MASTER BLISTER. Development of geosynclines, borderlands, and basins volcanism above blister with lengthening of crust.

Development of Crust Above Blister. A radial extension of the crust, which is caused by the expansion of the Blister, is accompanied by a circumferential lengthening at the surface. The magnitude, in any one direction, of this lengthening (ΔL) may be expressed by the formula $\Delta L = 2\pi\Delta R \cdot \frac{w}{40{,}000}$, where R is the difference in radius caused by the expansion; w is the width of the Blister parallel to the surface of the earth; and $\frac{w}{40{,}000}$ is that fraction of the entire earth's circumference occupied by the Blister. If, for example, we assume a radial expansion of 14 km. for a Blister whose width is 2000 km., the equation reads, by substitution, $\Delta L = 2 \cdot \frac{22}{7} \cdot 14 \cdot \frac{2{,}000}{40{,}000} =$ 4.4 km.

Surface rocks above the isothermic level are extremely likely to rupture when subjected to tension. When a Blister develops beneath the crust, and its increase in volume is accompanied by a radial expansion outward, the only possible adjustment to the circumferential lengthening demanded by the equation is through fissuring. The first fissures to develop would reach down to the roof of the blister, where the materials are in either a glassy or liquid state. The release of pressure in the former case would be adequate to cause a transformation to the liquid state. Consequently, in either case, magma would rise toward or to the surface, from the Blister area. The chemical nature of that magma would be key information as to the depth from which the magma rose. The loss of magma produces two important results. First, there is the decrease in Blister volume produced bythe loss of magma; and, second, there is the loss in Blister energy produced by the expansion and escape of the Blister materials. Each of these means loss of volume, and this is manifested by a radial shrinkage and pulling in of the Blister roof.

As the roof tends to collapse, two important changes which have transpired make a return to the original position impossible. The magma which was intruded into the fissures above the Blister might well tend to solidify in the fissures, thus increasing the length of the crust in that sector by an amount equal to the original circumferential lengthening. At the same time, the intruded and extruded material effectively decreases the thickness of the Blister, and the collapsing roof must settle down to a lower position than it had formerly, the total amount of the shrinkage being perhaps 125 per cent of the original radial extension. Returning to the example given earlier, where a circumferential lengthening of 4.4 km. was suggested, we find that a shrinkage of the radius to the roof of the blister of 1.25 × 14 km. means a circumferential shortening of 5.5 km. This circumferential shrinkage must be manifested by folding or/and overthrusting.

It is unlikely that a total collapse would take place as simply as this. It is rather to be imagined that the movement above the Blister will be comprised of a series of pulsations of expansions and collapses. In depth, the Blister grows wider and deeper constantly, as the roof passes through its oscillatory motions. Each expansion of the roof, therefore, is accompanied by a considerable broadening of the zone in which fissuring takes place. Since the crustal rocks can transmit compressional stresses much greater distances than they can tensional stresses, it is likely that the shrinkage and compression, which develop with each roof collapse, will be transmitted to whichever area happened to be sufficiently weak to yield to the compression of the first roof collapse. It is probable that this weak area would be a region somewhere out at the periphery of the Blister, where tension, during the first uplift, produced fissuring, but where the Blister materials were not sufficiently close to the surface to rise along the fissures and seal them, restoring their original strength. Thus, we see developing around some part or around the entire Blister a trough-like region. The width of this trough might well be a hundred or hundreds of kilometers, and the depth would continue to increase as long as the Blister pulsations continued. Successive uplifts of the Blister roof would be apt to produce a thinning or necking of the crust in the geosynclinal region.

The geosynclinal region might or might not be adjacent to a land mass. It might be in the sea or it might be on land. There is no strong reason for preferential development in any one type of topographic area or lithologic area. If sedimentation does occur, the weight of the sediments would augment the continued down-sinking of the trough. This prevailingly downward motion would, also, be increased by intrusions of basic magma, which would enter the geosynclinal sedimentary prism along fissures produced by an expanding phase of the neighboring blister. The nature of these intrusions would depend on the specific region in which the geosyncline developed and on the depth to which fissuring proceeded before the pressure release on the asthenospheric material was adequate to change that material to magma. Any down-sinking of the geosyncline would be accompanied by a stretching, continued necking, and weakening of the basement upon which the collecting sediments were resting (FIGURE 4).

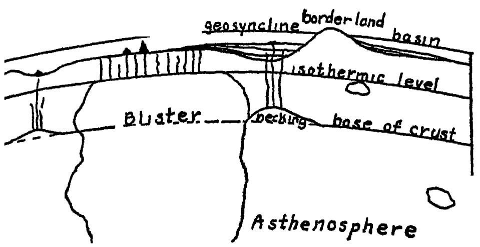

FIGURE 4. Geosyncline phase with volcanism into sediments Continued volcanism above blister.

Disturbances. It is obvious that after a geosyncline with sedimentation has been initiated, subsequent behaviour of the Blister roof must necessarily have a profound effect upon the bordering geosyncline. Each expansion of the roof will mean a continued stretching and down-sinking of the trough, and each collapse will mean a folding of part of the trough. Thus, a series of localized *disturbances* will develop, as, for example, in the overall history of the Alps or of the Coast Ranges. If the Blister be beneath the ocean, each expansion of the Blister roof would cause a flooding of the lands of the earth, and cyclical epicontinental sedimentation would accompany sedimentation in the trough.

Revolutions. The occurrence of a revolution, that is, the crumpling of most of the sediments of the geosyncline, does not occur, however, until very special conditions are obtained. The alternately expanding and contracting Blister must perforce grow laterally, since it cannot develop above the isothermic level. Eventually the heated Blister area will reach the basement region of the unstable geosyncline, which is capped by sediments with low heat conductivity. The thermal gradient becomes considerably steepened, and regional metamorphism of the lower geosynclinal sediments begins. It is important to realize that the recrystallization of the sediments is not the simple result of increased thermal gradient. The connate waters which are heated with the sediments act as media of molecular and ionic movement, and considerable migration is facilitated. The various zones of metamorphism depend, then, on the distance from the heat body and upon the amount of connate water present. In the presence of increased temperature, the waters will undergo considerable convection, which explains the enrichment or impoverishment of the various zones in various elements (FIGURE 5).

If the temperature of the connate waters approaches low temperature, magmatic conditions, a reversal of Bowen's reaction series takes place, and the alkali-, silica-rich portions of the rocks are dissolved therefrom, leaving the ferro-magnesiancalcic portions intact. If there be no migration of the newly dissolved materials, a *lit-par-lit* phenomenon results. But, if migration is induced by growing pressures in the geosynclinal area, wholesale granitization effects may be observed, and large quantities of hybrid magmas may be produced. Concomitantly with this process, ore substances which were present in the connate water and in the sediments are redissolved and form ore fluids in the new magma.

The existence of such large quantities of heated connate water in the geosynclinal prism further weakens the section, and the stage is set for a tremendous revolution upon the next collapse of the Blister. This collapse is hastened by the weakening of the geosynclinal sector, and is complete rather than partial, which was the case when magma was intruded into and upon the roof. By the time of the major collapse, the crust above the Blister may have lengthened by as much as 20 miles, and this lengthening must be manifested by a crumpling of the geosynclinal and bordering sediments. This shortening is augmented by vast quantities of palingenetic and asthenospheric magma which are forced into the geosyncline by the weight of the down-sinking Blister roof. The apparent shortening is further increased by the extensive thinning and flowage of the sediments in the zone of intense metamorphism.

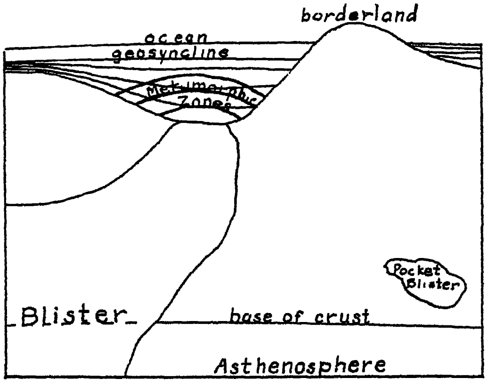

FIGURE 5. Beginning of metamorphism and preparation for orogeny

The compression is manifested in five ways (FIGURE 6). The inmoving basement from the Blister direction may slide beneath the surface of the basin deposits. This produces a *décollement* and a wrinkling of the sediments, resulting in marginal folds. Overthrusting on a large scale proceeds at the surface of the geosyncline, but, in depth, the rocks yield plastically because of their conditioning by heated connate waters. Any platy minerals which have been formed, or are forming, by recrystallization become alligned, either as growth response to pressure environment or by flowage thereafter. This alignment is perpendicular to the direction of greatest pressure. Most of the yielding by flowage will be geocentric, and roots of considerable magnitude will be forced into the asthenosphere. Not only the geosynclinal sediments but also the basement will be involved in this downward thrust of sialic materials into the heavier asthenosphere. Even the less dense upper portion of the asthenosphere may be forced downward. Large scale syntectonic magmatic intrusion is the fifth manifestation of the compression.

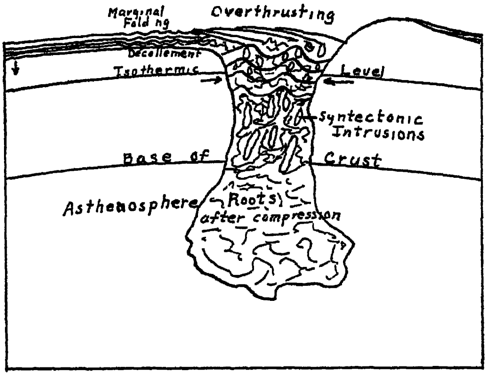

FIGURE 6. Compressional phase with formation of roots

Isostatic Adjustment. It is of little importance whether the down-forced lighter materials remain solid with strength or liquid without strength. They will serve as buoyant masses which tend to rise toward the surface. It is not to be expected that the entire down-forced area will rise uniformly. Rather, the brittle upper portion will break into a series of blocks, each of which will rise differentially. Post-tectonic intrusions of the discordant type will be developed prior to and during the uplift. During the first stage, these intrusions will be more acidic and of palingenetic origin. The later intrusions will be developed from deeper-seated origins and will be more basic (FIGURE 7). Of course, differentiation of any one magma chamber may result in a succession of progressively more acid intrusions.

The rising of the fault blocks, although the result of tension, will be along steep angle reverse faults, since this arrangement permits movement with the least amount of force; and, since the faults are of the reverse type, fractures will open and magma from depth will rise to the surface, producing the typical volcanism associated with fault block mountains.

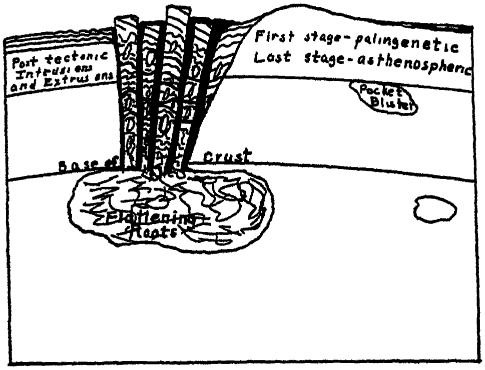

FIGURE 7. Isostatic uplift and fault block phase

During the first stage of isostatic uplift, uplift exceeds gradation, and high mountainous areas develop, the ruggedness of the mountains being primarily due to erosion. In the last stage, the rate of uplift is much less than the rate of gradation, the region begins to approach sea level once more, and a stage of quiescence is begun. At the same time, new Pocket Blisters have been forming, either in the same region as before or in other areas, and another cycle is in the making (FIGURE 8).

Possible Examples. The hypothesis is not developed as yet to such perfection that one can point with complete assurance to precise examples of the Blister Cycle. A few suggestions, however, may be made. During the Paleozoic Era, there were four borderlands contributing sediments inland into what is now North America and Greenland. Appalachia, Llanoria, Cascadia, and Arctica are now deep water, and inland from each of these there are now exposed ancient folded mountain belts. If these regions were blisters, the origins of the sediments, the geosynclines, and the folding could well be explained. Furthermore, the deep waters which now take their place may be explained on the basis of a considerable collapse of the Blister areas, the roofs of which were highly intruded by heavier magmas. If this thought is correct, the permanence of continents and ocean basins, except for the Pacific, must be greatly doubted.

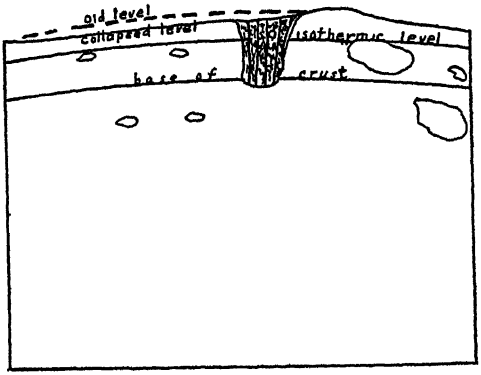

Figure 8 Erosion and quiescence with formation of new pocket blisters.

The history of the Paleozoic folding which surrounded the region of what is now the Atlantic is typical of what might be expected if a Blister in the Atlantic region were responsible for the geosynclines of the Paleozic Alps, the Scottish Highlands, the New England-Acadian area, and the Appalachian area. The Caledonian revolution began in late Silurian time in Scotland, followed by a somewhat later collapse in upper Devonian time in the New England-Acadian region, and in the Paleozoic Alps and Appalachian troughs in Permian time. A series of closely spaced orogenies in areas which form parts of a great arc around a common center is best explained on the basis of the growth and collapse of a Blister in that center. This may well explain the development of the great belt of negative anomalies, of mountains, and of volcanoes which now surround the Borneo region in the East Indies. According to the hypothesis, this belt is a recently compressed periphery of a Blister. The present phase represents an isostatic uplift of the root areas. The various depths of focus of quakes in adjacent areas depends in large measure to the degree of completion of isostatic uplift and on the magnitude of the root areas.

Africa represents an unusual continent in its high average altitude and in its rift line with associated volcanism. The existence of a blister beneath the African region is the most satisfactory explanation for these unusual phenomena. Generally speaking, surface volcanism is the result either of tension above a blister or of tension along reverse faults during isostatic uplift, and it is not very difficult to decide which of these two is operating in the various volcanic areas of the world today.

The great linear mountain ranges of the earth, such as the Andes, represent a folding of simple geosynclines. Such mountain complexes as the Himalayas, however, must be explained on the basis of the coalescence of possibly as many as three blister margins in one area, producing unusually large roots with the subsequent great uplift and complexity of development.

Other examples might well be urged at this point, but the above will suffice to give the general picture. Much study of a quantitative nature must be made on such problems as heat conductivity of rocks under various conditions of temperature and pressure and distribution of radioactivity in the crust. Paleogeography can be reconsidered in the light of the hypothesis and should prove a valid test for its proposals. The foregoing paper has been necessarily shortened, and many important details have been omitted, but the general principles have been outlined. The author is not completely satisfied, but in so far as he has been able to apply the hypothesis to the known facts of orogenesis, metamorphism, petrogenesis, and related geological phenomena, he is convinced that something of the sort must serve as the unifying principle which will integrate the many great problems of the crust.

SECTION OF BIOLOGY

THE TREATMENT OF HUMAN THYROID DISEASE WITH RADIOACTIVE IODINE

By J. B. TRUNNELL[*]

There is an irreducible minimum of nuclear physical knowledge and terminology which must be defined and understood before any intelligent discussion of clinical or other biological application of isotopes may be undertaken. *Isotopes* are substances having identical atomic numbers but different atomic weights. They may be stable or unstable. The latter are called radioactive.

It will be recalled that all atoms are composed of protons, neutrons, and electrons.[†] *Protons* and *neutrons* are collectively known as *nucleons* because together they make up the nuclei of atoms. Protons and neutrons (which have essentially the same mass) together contribute, for all practical purposes, the *atomic weight*. Neutrons bear no charge. Although much "heavier" (1838 times) than an electron, the positive charge of a proton exactly counterbalances the negative charge of an electron. The number of protons in the nucleus of an atom (and consequently the number of electrons around the nucleus required to balance the charge) is known as the *atomic number*. Factors closely concerned with the atomic number determine the chemical characteristics of a substance. The atomic weight of an atom may vary (according to the number of neutrons) without changing the chemical characteristics, provided the atomic number is not altered.

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[*] Memorial Hospital, New York, N. Y. This paper was the first of three papers presented on the subject, *Biological and Clinical Studies with Radioactive Iodine*, at the meeting of the Section on March 14, 1949.

[†] No doubt a gross oversimplification, but convenient for descriptive purposes

The element hydrogen, with its isotopes, provides a good example of these phenomena. Hydrogen has an atomic number of one and an atomic weight of one, since its nucleus is composed only of one proton. Certain of its chemical characteristics are well known and include the ability to combine with oxygen or halogens to form water or acids. If a neutron is added to the nucleus of a hydrogen atom, the atomic number remains unchanged, but the atomic weight is doubled. The resulting atom will be a stable isotope of hydrogen known as deuterium or heavy hydrogen. This substance is chemically indistinguishable from ordinary hydrogen and participates in chemical reactions exactly as does hydrogen. Deuterium is a stable isotope, since the nuclear configuration does not change spontaneously. If, on the other hand, an additional neutron is added to the nucleus, thereby increasing the atomic weight to three, while preserving the atomic number of one, still another isotope of hydrogen is formed. This nuclear composition is not "satisfactory" and will re-arrange itself to achieve a more stable ratio of protons to neutrons. In the process of re-arrangement, a particle is ejected from the nucleus at high speed.

For the sake of convenience, a neutron can be thought of as dividing into a proton and electron. However, the presence of an electron in the nucleus itself is apparently not tolerated. Consequently, as soon as the electron is formed, it is forcibly ejected at a speed which may approach the velocity of light and is known as a beta particle or a beta ray. A few millionths of a second later (not in radioactive hydrogen, but in many radio isotopes), there may also be emitted a packet or quantum of energy in wave form known as a gamma ray. The residual proton obviously increases the atomic number by one. Hence, the atom is no longer one of hydrogen, but has become an isotope of helium, the only element which has an atomic number of two.

The term *curie*, as employed in isotope parlance, defines a quantity of potentially radioactive atoms in terms of a rate of nuclear transformation, which rate is, numercially, 3.7×10^{10} transformations per second. A millicurie is 1/1,000th as much (the quantity in which there are 3.7×10^{7} transformations per second), whereas a microcurie is 1/1,000,000th as much (3.7×10^{4} t/s).

Half life is a convenient phrase designating the period during which one-half of a given large quantity of potentially radioactive atoms will have undergone nuclear transformation. Iodine 131, for example, has a half life of eight days, which means that of any quantity measured on this date, only half the quantity will remain after eight days. After sixteen days, one fourth will remain. After twenty-four days, one eighth will still have failed to undergo nuclear transformation. The half life of any given isotope cannot be influenced by any alterations of temperature or chemical combinations, unless the nuclei of the atoms themselves are changed (as may occur in a carbon uranium pile or in a cyclotron). The half life period may vary enormously from one element to another or among isotopes of the same element. Isotopes are known which have a half life of only a fraction of a second, whereas others have half lives of many thousands of years.

Specific activity is an expression of the number of millicuries (or curies) per unit weight of material. The specific activity of some isotopes is impressively high. For example, the largest single therapeutic dose of radioactive iodine, 315 millicuries, weighed approximately 2.5/1,000,000 of a gram. This quantity, which is too small to visualize without the aid of optical equipment, is only slightly greater than the amount of stable iodine which is normally present in a tumblerful of New York City tap water.

Almost the entire usefulness of radioactive isotopes in tracer or therapeutic studies depends upon three fundamental characteristics:

(1) Because isotopes have identical atomic numbers and hence identical chemical behavior, radioactive atoms of a given substance will undergo the same chemical reactions as the more familiar stable isotopes. Hence, other chemical substances and even biological systems cannot differentiate one isotope from another, and will utilize atoms of each in direct proportion to their percentage composition in any given mixture of two or more.

(2) Radioactive atoms can be detected conveniently by means of relatively simple physical equipment. Depending upon whether or not gamma rays (long range) or beta rays (short range) are emitted, detection can be accomplished at a point outside the intact body. Highly accurate quantitation of quantity per unit volume or unit weight is usually possible.

(3) The speeding particles emitted from the nucleus produce effects in tissue which are, in the final analysis, similar to the effect produced by X-rays or by the gamma rays of radium. It is well to recall that X-rays or gamma rays do not primarily influence tissue, but do so indirectly by knocking high speed electrons from atoms of tissue itself. These electrons, being capable of ionizing molecules through which they pass, produce tissue damage which, within rather well defined limits, is a function of the amount of energy absorbed by the tissue.

This amount of energy absorbed has been used as a basis for calculation by Marinelli of a tissue dose in equivalent roentgen units for a variety of isotopes. Iodine 131, which has been used most extensively in these studies, delivers 158[*] equivalent roentgen units per microcurie of iodine per gram of tissue (assuming that there is homogeneous distribution of the isotope in the tissue and that the isotope remains during its entire duration of decay). It can be said, therefore, that, theoretically, very large quantities of radiation may be delivered to tissue when a radioactive isotope is selectively absorbed and retained. For example, if 500 microcuries of Iodine 131 were to be deposited in each gram of a given quantity of thyroid tissue, 79,000 equivalent roentgens would be delivered in the tissue, providing the iodine was not metabolized and secreted.

Several factors operate, however, to prevent the actual realization of such tissue doses. First, and perhaps most important, there is not homogenous distribution of the isotope in the tissue. This spotty type of distribution can best be seen by examining a radioautograph.[†] Secondly, thyroid cancer which is differentiated well enough to collect iodine is, also, usually capable of making some thyroid hormone or a reasonable facsimile thereof. Accordingly, iodine which is collected does not remain in the tissue during its entire period of physical decay, and upon being secreted is obviously no longer capable of radiating the tissue. Hence, the calculated dose is diminished further. A third, and equally important factor, has to do with the fact that cells at the periphery of any mass of tissue are not being radiated from all adjacent cells, as are those deeper in the tumor. Hence, the calculated dose is again reduced in these areas by a factor of at least two. Despite these limitations, impressive quantities of radiation can be and have been delivered to normal thyroid, hyperfunctioning thyroid, and malignant thyroid tissue.

Until fairly recently, radioactive Iodine 131 was prepared by neutron bombardment of tellurium with subsequent chemical separation of the two materials. Now, however, it has been found feasible to separate radioactive iodine from other uranium fission products as a by-product of pile operation. Radioactive iodine thus prepared is carrier free (that is, there is no admixture with stable iodine) and, in recent months, has become tellurium free also. There is, however, some contamination with another radio isotope of Iodine 133. This isotope, however, has a short half life of only 22 hours, and the degree of contamination is not great, particularly after time has elapsed in transit. Shipment is routinely by air freight, and the material arrives safely enclosed in lead-shielded containers. Iodine 131 is supplied as sodium iodide already in solution in distilled water.

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[*] This figure used by Quimby to replace older value of 147 equivalent roentgens.

[†] Made by excising tissue from a patient previously given radioactive iodine, fixing and sectioning in the usual way, and, then, exposing radiation sensitive (photographic) emulsion. After an appropriate period of exposure, the photographic emulsion can be "developed" and will show areas of blackening which correspond to areas of radio isotope concentration in the tissue section.

After appropriate dilution, the material can be administered to animals or patients either by injection or orally. For the vast majority of human administrations, the oral route is preferable, although, for certain types of studies involving iodine metabolism, more reproducable results can be obtained by intravenous injection.

As has already been pointed out, the quantity of iodine in solution at the time of administration, even with large doses, is very small. Consequently, there is no taste, and the patient is unaware that he or she is taking anything but plain water. Before iodine was supplied from fission in the pile, there was appreciable contamination with the element tellurium. This substance imparts a characteristic garlic-like odor to the breath of persons receiving therapeutic quantities, but, is, of course, no longer a contaminant.

In so far as is known, no organ or tissue has the same high degree of selective concentrating ability for any element or compound as that exhibited by the thyroid for iodine. It is not at all surprising, therefore, that as soon as radio isotopes of iodine were available, they were eagerly pounced upon by investigators of thyroid physiology. Since that time, there has been rapid progress both in the acquisition of knowledge regarding iodine metabolism and in the evaluation of radioactive iodine as a therapeutic tool.

The treatment of hyperthyroidism can be accomplished with much smaller doses than those required in cancer. Because of the very nature of gland activity in hyperthyroidism, the tissue collects much more iodine than does a normal or neoplastic thyroid. Hence, in a general sense, the overall dose required is an inverse function of the rate of turnover of iodine, and is a direct function of the total mass of tissue to be irradiated. Depending upon the degree of hyperfunction and gland size, satisfactory control of hyperthyroidism can usually be accomplished with doses of from four to twelve millicuries of Iodine 131. Doses employed in the treatment of thyroid cancer, however, range from fifty to three hundred or more millicuries. In both conditions, it is preferable to have patients receive a low iodine diet for several weeks prior to treatment, in order that undue dilution with stable isotopes be avoided. Otherwise, the urinary excretion of the administered material will be unnecessarily high. If the patient has been receiving any of the antithyroid drugs, they should be discontinued for forty-eight hours before preliminary testing or before therapy.

A rapid survey of the conditions of iodine collection and renal excretion in any given patient can usually be accomplished by giving a small "excretory" dose, usually about twenty microcuries by mouth. Complete collections of urine in twenty-four hour periods for two days should then be made. Measurement of the quantity of radioactive iodine excreted provides a good clue because of the reciprocal relationship existing between collection by iodine metabolizing tissues and the renal excretion of iodide. Misleading results will be obtained if the patients have been receiving iodine or iodine-containing compounds, such as those used for gall bladder visualization, in which case the urinary excretion of iodine will be falsely high.

In the event of hyperthyroidism, therapy may proceed with only the urinary excretion data and an estimate of the gland size as determinants of dose size. According to Chapman, one should endeavor to deposit about 150 microcuries per gram of estimated weight of thyroid tissue. Hence, in an individual with a gland estimated to weigh 50 grams, who has shown by a previous excretory dose that the urinary excretion will be 15 per cent, the total oral dose to be given can be calculated by multiplying 50 by 150 and by adding the 1.4 millicurie to be lost in the urine. The assumption is made, of course, that the 7,500 microcuries remaining in the body will all be in the thyroid. This is not strictly true, but, by setting 150 microcuries per gram as a minimum, sufficient allowance has been made to permit the delivery of adequate radiation in the gland. Following dosage of this type, the patient may be expected to achieve a euthyroid state after about six weeks.

Though enthusiasm varies from one institution to another for the use of radioactive iodine in the treatment of Graves' disease, there are sufficient theoretical considerations to argue against its widespread employment, particularly when surgical management of the disease in patients properly prepared with thiouracil and iodine has become eminently satisfactory in a great majority of cases. The possibility of the development of neoplasms fifteen or twenty years after radioactive iodine therapy should preclude its use in all but elderly or otherwise bad risk patients for surgery. There are, no doubt, isolated cases in which unusual circumstances also justify use of Iodine 131. A somewhat different philosophy is permitted in dealing with cancer of the thyroid, although this term connotes variations between such wide limits of morbidity and mortality that much room for controversy exists.

There is almost no doubt however that radioactive iodine should *not* be employed as the only or first therapy in early or uncomplicated cases of thyroid malignancy. Cancer which is localized to the neck, even though extensive node metastases may be present, should be treated by radical surgery. Radioactive iodine, with rare exceptions, should be limited to those cases in which there are extensive or distant metastases in inaccessible places.

Of primary importance is the matter of whether or not these metastases collect iodine. There is some difference between figures obtainable from radio autographic studies (50%) and those obtainable from external body measurements (15–20%), with regard to percentage of otherwise suitable patients who are initially treatable with radio iodine. Although the former figure is higher, there is now adequate demonstration of the fact that a number of procedures are available not only to increase the latter figure, but, also, to increase the amount of iodine collected. These will be described below.

Any patient in whom isotope therapy of this type is contemplated should undergo a meticulous and complete clinical and laboratory examination. One should not rely upon the Geiger tube to locate metastases. Instead, a complete radiographic survey should be made. Repeated counts of red cells, white cells, total and differential, platelets, reticulocytes, hemoglobin, hematocrit, sedimentation rate, sternal marrow examinations, cholesterol, basal metabolic rate, and blood iodine levels should be made. Iodine intake should be restricted, but the ubiquitous distribution of iodine in food makes an iodine-free diet highly impracticable. Sea foods are specifically prohibited, and non-iodized salt should be used.

A small "excretory" dose of only a few microcuries (depending upon sensitivity of counting equipment available) can be given to determine the expected loss of larger doses of iodine in the urine. When this figure has been determined, a test dose of 200 – 500 microcuries should be given. Forty-eight hours after giving such a dose, measurements can be made over previously localized metastases. The major difficulty in determining the overall size of the therapeutic dose to be given lies in the lack of knowledge concerning the minimum cancercidal dose for various types of thyroid tumors.

Experience with X-ray therapy provides a rough basis for estimating the ideal dose goal. Certainly, where possible, one should endeavor to deliver no less than a total of

6,000 roentgens. Difficulties already described in achieving uniform distribution of radiation in these tumors do not permit uncorrected use of the expression "158 equivalent roentgens per microcurie per gram." Allowance must be made for spotty distribution of the isotope, for "edge effects," and for metabolism and secretion of iodine, all of which tend to reduce radiation in some or all areas of the tumors. No precise opinion can be expressed to cover all contingencies. However, if one even hopes for "destruction" of malignant tissue, a factor of at least six should probably be applied. Hence, instead of 6,000 roentgens, a more proper goal may be approximately 40,000 equivalent roentgens. Therapy is no doubt worth undertaking if these figures seem unattainable, but hope of tumor ablation should be guarded. Final calculation of the total oral dose depends, as in hyperthyroidism, upon previously determined renal excretion and upon estimated tumor weight and measured tumor iodine collection. In a hypothetical case in whom renal excretion for the first ninety-six hours totals 45 per cent and in whom exists an estimated one hundred grams of metastatic cancer, which has been shown by external readings to collect approximately 15 per cent of a test dose, the following considerations are of importance. To deliver 40,000 equivalent roentgens, it will be necessary to deposit about 250 microcuries per gram of estimated tumor weight. The entire quantity of Iodine 131 to be taken up by the aggregate metastases is then 100 × 250 microcuries or twenty-five millicuries. The total dose is then $\frac{25 \times 100}{15}$ or 166 millicuries. This is within the range of doses which have been given without immediate or apparent undue effects on normal tissues (single doses of 315 have been given). However, the above case represents a quite favorable setting seldom encountered.

In less suitable cases, it seems advisable to attempt to improve the ability of the metastases to collect iodine, even though several months may elapse prior to the administration of Iodine 131 therapeutically.

Four procedures have shown more or less promise as means of improving iodine collection:

(1) Thyroidectomy (either surgical or "medical" with radioactive iodine) has been followed in twelve of twenty-four patients by significant increases. Improvement in isotope pick-up is not sudden, but continues slowly to increase for several months. In some instances, the degree of increase has made therapy possible in patients previously unsuitable.

"Medical" thyroidectomy with radio iodine should be employed only in instances in which surgery is strictly contraindicated, since the latter is quicker, more certain, and does not use up any of the patient's overall total tolerance for Iodine 131. Further, considerable uncertainty surrounds the minimum effective dose for destruction of thyroid function. Doses as small as 15 millicuries have succeeded and doses as large as 70 millicuries have failed.

(2) Administration of relatively pure thyrotrophin* in doses of 30 milligrams per day has also been shown to increase collection of iodine and to diminish urinary excretion. The increase may be by as much as a factor of four or more. Therapeutic dose administration should be at forty-eight hours after discontinuing thyrotrophin, otherwise, a strongly refractory state may be encountered during which iodine collection is diminished, often to zero.

(3) Recently, very encouraging results have been obtained in a small number of patients given thiouracil for long periods. In one individual so treated, collection of iodine by multiple pulmonary metastases rose to more than 90 per cent of the total administered dose.

(4) Prolonged high grade water diuresis (suggested by Dr.s Alfred Gilman and Louis S. Goodman) has resulted in decreased urinary excretion and increased tumor pickup. Such diuresis must continue for a week and the daily intake needs to be

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* Kindly supplied by Dr. John Mote of the Armour Laboratories, Chicago, Illinois.

kept at a minimum of eight liters. Iodine should be given, of course, just as increased urinary output has subsided after returning to normal fluid intake.

Results of therapy are difficult to interpret in the light of the relatively few cases which have been vigorously treated. No two have been handled exactly alike and, almost certainly, the maximum possible quantity of radiation has not been delivered in anyone. Repeated doses have been given to several patients, so that their total amount of ingested radio iodine approaches one curie.

Unequivocal improvement has been observed in a sufficient number of these patients to indicate the fundamental soundness and potential usefulness of the technique. Major effort is still being concentrated upon ways and means of improving iodine collection by these tumors, since the few really striking results have been seen only in patients with cancer of high concentrating ability. Decrease in pain and narcotic requirement, regression in varying degrees of palpable tumor, improvement in appetite and "general well being," gain in weight, and improvement in vision when impaired by lesions in the skull are among the more definite changes seen in one or more patients.

Complications and damage to normal tissues have been extensively anticipated, and, though evidence for their occurrence exists, it is perhaps more meager than originally feared. One patient, in generally poor condition, died with pancytopenia following several large doses of radio iodine. Others have shown varying degrees of bone marrow and peripheral blood depression. In some, despite substantial quantities of radiation, the bone marrow has returned to an approximately normal morphology. Three female patients in their late thirties have ceased to menstruate after treatment of thyroid cancer with radio iodine. No definite evidence is available to suggest damage to liver, kidneys, or other organs. Little comfort should be derived from the present paucity of normal tissue damage, however, since the long latent period of radiation damage onset is well known.

An acute thyrotoxicosis has been observed to follow treatment in several patients. This complication is presumed to result from sudden release of previously stored hormone when confining cells are destroyed by radiation.

In individuals with low cardiac reserve, this constitutes a real hazard. Such individuals should probably be given thiouracil for two months, in the hope that, thereby, most of the hormone stored in the tumor may be gradually released before radiation is given.

The entire subject is still in the earliest stage of development. Experience, even in the busiest centers of investigation, is woefully inadequate for the formulation of conclusions or of hard and fast opinions. All that has been expressed in this review is, therefore, subject to revision. Time and continuing investigation will provide more tenable points of view in this currently limited, but potentially large field of therapeutics.

DEGENERATIVE AND REGENERATIVE CHANGES IN THE THYROID GLAND FOLLOWING HIGH DOSAGES OF RADIOACTIVE IODINE

By AUBREY GORBMAN[*]

The employment of radioiodine as an injurious agent for control of thyroidal overactivity or cancer has prompted interest in the degree of thyroidal injury to be expected from a given amount of I^{131}. Another significant possibility attending use of this radioisotope is the injury of neighboring tissue by radiation emanating from the thyroid gland.

For experimental study of this problem, C_{57}- and CFW-strain mice weighing between twenty to twenty-five grams, whose thyroids had an average weight of about five milligrams, were used. They were injected subcutaneously with 100, 200, 400, or 1,000 microcuries of I^{131}. In a separate study, it was found that, under conditions of diet and maintenance in our laboratory, uptake of injected radioiodine by the thyroid was between 5 per cent and 10 per cent, and reached a maximum between 12 and 24 hours, for the 100 and 200 microcurie doses. No such study has been made with the higher doses. After reaching a maximum value, the thyroidal I^{131} concentration was dissipated at slightly different rates, depending on the dose given. The "biological half-life" in the thyroid was about two days. Calculated roughly from such data, and as though the mouse thyroid were a large piece of tissue, the radiation dose was about 26,000 equivalent roentgens for the 100 μc. dose and 75,000 e. r. for the 200 μc. dose. Since the dimensions of the mouse thyroid are of the same order as the travelling range in tissue of β radiation from I^{131}, much of the β ionization is in neighboring tissue. Consequently, the above calculation of intrathyroidal equivalent roentgens delivered by a certain concentration of I^{131} is probably much higher than actual for the mouse thyroid, which in no place is more than a millimeter thick.

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[*] Department of Zoology, Barnard College, Columbia University, N. Y. This paper was the second of three papers presented on the subject, *Biological and Clinical Studies with Radioactive Iodine*, at the meeting of the Section on March 14, 1949. This work was aided by a grant from the National Cancer Institute, U. S. Public Health Service, and by contract with the Atomic Energy Commission and the Office of Naval Research.

Mice were sacrificed at intervals of 2, 3, 7, 24, 120, and 300 or more days after administration of radioiodine. Animals given 1000 microcuries never survived beyond one week. Those given 400 microcuries usually died before one month. All thyroid glands were removed in contact with the trachea and were completely sectioned serially. A few of the long-term animals were given a second tracer dose of 10 microcuries 24 hours before autopsy and radioautographs were made of sections through the thyroid region.

Short Term Effects. The severity of injury to the thyroid after two or three days varied from minimal to complete destruction within the 100–1000 μc. range of dosages. The 100 μc. animals killed after 2–3 days (having yet received only a fraction of the total possible radiation) revealed thyroidal injury only upon careful study. In these, small centers of pycnosis of follicular cells appeared, especially in the medullary part of the gland. Parathyroid was apparently uninjured, but any thymic tissue near the thyroid was at least partly degenerate.

The 200–300 μc. dosages produced more extensive epithelial pycnosis as well as vacuolization. Many thyroidal cells were enlarged and contained large droplets of material similar to the colloid. Intrafollicular bleeding was not uncommon. Constant accompanying phenomena were periglandular edema and swelling of endothelial cells of intrathyroidal blood vessels.

The 1000 μc. dose at two days produced complete destruction of the thyroid and parathyroid tissue. Since destruction was so rapid, the follicular architecture of the thyroid was preserved, but all material, former cells and colloid alike, were amorphous in texture and uniformly eosinophilic. Only in a few places could very lightly staining nuclei be recognized. All tissues in the walls of thyroidal blood vessels were hyalinized and no stainable nuclei were evident. Yet, such blood vessels appeared to be carrying blood successfully. At seven days, active phagocytosis had removed most of the cellular debris resulting from such destruction and fibrotic changes, shrinkage, and small hemorrhages were evident.

Twenty-four-day Interval. Examination at this interval shows surviving thyroidal tissue only in the 100 μc.- and 200 μc.-dose mice. At this time, it should be remembered that virtually all of the ionizing radiation has been expended. Tissue surviving the 200 μc. dose is restricted to the cranial apex and the isthmus of the thyroid. Most of it is pycnotic, but some cells show a greatly hypertrophied cytoplasm despite variable degrees of nuclear pycnosis. Colloid next to such cells usually is well vacuolated, a change which is usually taken as a sign of secretory activity. The main part of the thyroid is represented by a shrunken fibrous band, the edema having subsided. The 100 μc. dose permits survival, at this time, of a larger amount of thyroid, some even in the "body" of the thyroid, but only at the edge of the gland. A smaller proportion of cells is distinguishably pycnotic and more are in the hyperactive state.

120 to 400-day Interval. After 120 days, surviving thyroid is found only in 100 μc. animals. This is found as one or two small patches of follicles in a shrunken fibrous band of tissue. Surviving thyroid is very atypical in appearance. It is organized in small follicles or as nonfollicular masses. The cells are enlarged and of odd shapes and usually contain colloid-like masses. It is interesting that there is little or no difference between surviving thyroid masses of 120 days and those of 400 days, indicating absence of any tendency toward regeneration. Radioautographs showed that these glands do metabolize some iodine, but preliminary tracer studies indicated that the amount of iodine retained is relatively small. The relation of this amount to the mass of surviving tissue is difficult to determine. No neoplastic changes have been observed.

Summary. Under specified circumstances, 200 μc. or more of I^{131} cause complete thyroidal destruction in mice. Doses of 100 μc. permit survival of small bits of atypical thyroidal tissue, which even after 400 days show no tendency toward regeneration.

THE ZONE OF ACTIVITY OF ANTIBODIES AS DETERMINED BY THE USE OF RADIOACTIVE TRACERS

By DAVID PRESSMAN[*]

Since the turn of the century, attempts have been made to use antisera prepared against malignant tissues in the treatment of cancer.[1] Such antisera prepared against infectious agents have been found effective in combating the corresponding diseases, and it was reasoned that antisera prepared against cancerous tissue might be effective in combating cancer.

The sera were produced by injecting tissue homogenates or fractions of tissues into rabbits. The rabbits were then bled and the serum thus obtained used therapeutically. Unfortunately, the results obtained by the original investigators have been quite equivocal. The failures to obtain positive results may have been due to one or more reasons: (1) The serum contained no antibodies which would react with any part of the tissue. This is unlikely, since antibodies of some sort can usually be obtained by such a procedure. (2) The sera contained antibodies which cross-reacted with other tissues and were completely taken up by the other tissues. (3) The sera contained specific antibodies for components inside the tumor cells, but could not reach these intracellular components and effect them. (4) The sera contained antibodies which did localize in the tissue, but did not produce an observable physiological effect, and, since the results were actually dependent upon a physiological effect of the antiserum, negative results were recorded.

Actually, with transplanted tumors, it has been shown by Kidd,[2] Burmeister,[3] and Green[4] that exposure of the tissue to its homologous antisera before transplanting destroyed the ability of tumor tissue to propagate, and, here, we have evidence of a physiological effect of an antiserum.

In a reinvestigation of the field of tumor immunology, it seems reasonable that, first, it must be proven that there is something in an antiserum prepared against a malignant tissue which goes to the tissue in question. This can be accomplished by the use of radioactive tracers. If an antiserum against the tissue is prepared and the antibodies are tagged by coupling with radioactive substances or by having a radioactive elements incorporated in the antibody molecule, disposition of the antibody, after its injection, can be traced by following the disposition of radioactivity. Further, it is not impossible that, if antibodies can be found which go specifically to a certain tissue, they can be made to carry physiologically active amounts of radioactivity to the tissue. Thus, reliance need not be placed upon the capacity of the antibodies themselves to produce a physiological effect.

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[*] The Sloan-Kettering Institute for Cancer Research, New York, N.Y. This paper was the last of three papers presented on the subject, *Biological and Clinical Studies with Radioactive Iodine*, at the meeting of the Section on March 14, 1949.

Evidence has been obtained[5-7] that other diseases, such as glomerulonephritis, rheumatic fever, and multiple sclerosis, may be caused by the production of antibodies by an animal against his own tissues. The use of radioantibodies may well be important in tracing the course of these diseases. We have been able to prepare an antikidney serum and follow its course to the kidney. This is the work which I shall report here.[8-10]

The Effect of Coupling Various Substances to Antibodies on the Antibody Specificity

The fact that various substances can be coupled to antibodies without destroying the ability of the latter to combine with antigen has been shown previously.[11-15] Thus, as early as 1930, Reiner[11] showed that arsanilic acid can be diazotized and coupled to antibodies without destroying antibody specificity. It is not too surprising that this is possible, since the antibody molecule is very large, with a molecular weight of about 160,000, and the antibody specific region is, probably, just a small part of the surface of the molecule. Marrack[12] was able to couple a red dye to antityphoid serum and showed that the typhoid bacteria, when agglutinated with the antiserum, were colored red. The serum was labelled by coupling "R-salt" to one end of bisdiazotized benzidine and the serum to the other. Later, Coons, Creech, and Jones[13] prepared a labeled antipneumococcus type 3 serum by coupling the antiserum with β-anthrylisocyanate to form a serum containing β - anthrylcarbamido groups. Pneumococcus type 3 organisms, when agglutinated with this serum, showed a blue fluoresence. Then Coons, Creech, Jones, and Berliner[11] coupled fluorescein-4-isocyanate with antiserum against type 3 pneumococcus, and the pneumococci, agglutinated, showed a green fluorescence. They were able to show the presence of pneumococcus type 3 in tissues by means of this antiserum. Their technique was to sacrifice an animal heavily infected with pneumococcus type 3, prepare slides of the various tissues and stain the slides with the fluoresceins labeled antiserum. The regions of the tissue which showed the green fluorescence were considered to be those containing either the pneumococci or the specific polysaccharide. In all the labelings so far described, there has been a known definite chemical bond formed between the antibody and the material coupled. For instance, the diazotized compound coupled with the tyrosine and with the histidine to form an azo linkage and the isocyanates coupled with free amino groups of the protein to form carbamido linkages. McClintock and Friedman[15] "combined" uranium and malachite green with antibodies against pneumococcus polysaccharide and showed localization of the serum in regions which had been heavily infiltered with the pneumococcus polysaccharide.

For our work, we decided on the use of radioiodine 131 as a tracer. It has the advantages that iodine will directly iodinate the tyrosine and, probably, the histidine of a protein molecule, and this forms a definite bond with the protein molecule. It is easily obtained, easily counted, and has a convenient half life. Iodine has the disadvantage that it goes to the thyroid, but it is known that this can be controlled by feeding non-radioactive potassium iodide.

We have shown that it is possible to iodinate an antiserum, without destroying its specificity, by iodinating antiserum prepared against ovalbumin. That the antiovalbumin activity has not been destroyed is evidenced by the fact that such antiserum is precipitable by the specific antigen. That the antibodies in an iodinated antiserum actually contain iodine and that the radioactivity is specifically precipitated are demonstrated by the following experiment: Antiovalbumin serum was iodinated by iodine containing a tracer amount of iodine 131. This iodinated antiserum was then mixed with antiserum prepared against beef serum coupled with diazotized para-aminobenzoic acid.[16] The resulting mixture was divided into two parts: to one was added ovalbumin; and to the other was added beef-serum azobenzoic acid. Both antigens were in the concentrations for optimum precipitation. Precipitates were obtained in both cases. In the case of the ovalbumin precipitate, there was a high level of radioactivity, and in the other precipitate there was practically no activity. This showed that out of the mixture something that was radioactive was brought down specifically by ovalbumin and not non-specifically by a non-specific system. Thus, the conclusion must be drawn that the antibody specificity was not destroyed and that the antibody molecules specific to the ovalbumin contained iodine.

The Preparation of an Antiserum Against a Tissue and the Tracing of the Antibody to the Tissue

The next step was the preparation of an antiserum against the tissue and tracing it to that tissue. For carrying out this step in our method, we selected to study antiserum which can be prepared against rat kidney. This antiserum was studied previously by Masugi,[17] and later, by Smadel and his coworkers,[18] and has been shown by these workers to possess definite nephrotoxic effects and to produce physiological changes in the structure of the rat kidney when administered intravenously into the rat. Since such a serum has definite effects on the kidney, it seemed logical that the antibody probably goes to the kidney, and, if the antibody were labeled, it should be possible to follow it to the kidney. We carried out this experiment by preparing such an antiserum against kidney tissue, according to the method of Smadel, and then separating out the globulin by fractional alcohol precipitation and subsequently iodinating with iodine containing tracer concentrations of I 131. By injecting several rats with this radio anti-kidney globulin and others with a similarly prepared radio antiovalbumin globulin as controls, waiting for about 4 days and then sacrificing the animals and analysing the various tissues, we found that there was a definite accumulation of iodine in the kidney. With anti-kidney serum, there is a very large uptake of radioactivity by the kidney, whereas with the antiovalbumin, there is practically no uptake in the kidney. This shows definitely that there is something present in the anti-kidney serum, presumably an antibody, and absent in antiovalbumin serum, which localizes in the kidney.

However, there is increased uptake by other organs when anti-kidney serum is administered, but to a much lower extent than is the increase by the kidney. This cross reaction is probably due to the presence of common types of tissue in the various organs, such as blood vessels, *etc.* There remains the possibility that the pickup of the radioiodine by the kidney is not directly due to the radio antibodies remaining in the kidney, but that it may be due to a non-specific effect of the anti-kidney serum to sensitize the kidney so that it can nonspecifically pick up various substances, such as radio-antibody or the radio-iodinated proteins other than antibody, which are injected at the same time. This possibility, although it seemed unlikely, was investigated and was found not to be the case.[9] This was demonstrated by inoculating one group of animals with a radio-iodinated anti-kidney serum, another group with radio-iodinated antiovalbumin serum, and a third group with a mixture of radio-iodinated antiovalbumin serum and a non-radioactive, but iodinated, anti-kidney serum. There was a pickup of radioiodine in the kidneys of the first group, but not in the second and third groups, showing that the non-radioactive kidney serum did not cause an alteration of the kidney so that it would pick up the radio ovalbumin.

We have since repeated our localization experiments using mice, with much the same results. We have further shown that the localization of the anti-kidney bodies is not a strain specific phenomenon, since the antiserum prepared against the kidneys of AKM mice will also localize in the kidneys of Rockefeller Swiss mice, A strain mice, C57 black mice, and Rockland Farm mice. Cross reaction with rat kidney has also been observed.[10]

Recently, we have obtained data concerning the duration of antikidney serum in the kidney as compared with other tissues. We have measured the rate at which the radioactivity in the various tissues disappears for a length of 54 days. When plots were made of the logarithm of the radioactivity concentration in the tissue against the time plotted lineally, straight lines were obtained in most cases. It was possible to follow the radioactivity in these experiments in the kidney for a period of 54 days and in the thyroid for 54 days, but the concentrations in radioactivity in the blood stream, spleen, and liver decreased too rapidly to permit the measurement over the long range of time. From the straight line curve, it was possible to calculate half-life for the retention of the carrier of the radioactivity. The half-life of the carrier in the kidney was 20 days; in spleen, 2.6 days; liver, 2.9 days; blood, 1.8 days; thyroid, 6.8 days; and the whole animal, 2.0 days. The long half-life of the radioactivity carrier, presumably the antibody, in the kidney, indicates that, apparently, the antibody has become attached to the cell in such a way that the enzymes of the cell or of the tissues cannot metabolize the antibody. This prolonged retention of radioactivity is greater even than the retention of iodine by the thyroid, and emphasizes the possibility of a radio therapy of cancer if specific antibodies can be found, since the duration of the carrier is so prolonged. The preparations of iodinated antibody which we have been using contain about 0.5 per cent of iodine. This means that 1 mg. of antibody protein, which can localize in the tissue, carries 5 micrograms of iodine and corresponds to about 600 millicuries of iodine, if all the iodine were radioiodine.

In order to determine the structures in the kidney which pick up the radioantibody, Miss Hill and Dr. Foote and I prepared radioautographs[19] of kidney sections obtained from mice inoculated with radioactive anti-kidney serum, in one case, and radioactive anti-mouse plasma serum, in the other case. There were definite regions of radioactivity concentrations in the kidneys of the anti-kidney serum-treated animals and no concentration with control animals. The radioactivity seems to have accumulated in a small punctate region, and we have found that the region corresponds to the glomerulus.

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DIVISION OF MYCOLOGY

THE ANTI-BORRELIA EFFECT OF BORRELIDIN

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R. J. SCHNITZER*

It has been mentioned in the papers by Goldberg, Berger, and Jampolsky[1, 2, 2a] that the new antibiotic found and isolated by these investigators exerts a remarkably specific effect on borrelia, the spirochetes of relapsing fever. This activity seemed sufficiently impressive to give the name "borrelidin" to the newly found agent.

The purpose of the following technical remarks is to explain the experimental findings which support the facts presented by Goldberg and his associates.

As is well known, the borrelia infection of mice is not easily influenced by chemotherapeutic drugs. For many years, certain arsenicals and gold compounds were the only known substances capable of controlling an experimental borrelia infection. The high activity of penicillin in borrelia infections was first described, in 1943, by Heilman and Herrel[3] and has been confirmed by many authors since then. Recently, Heilman[4] found a similarly striking anti-borrelia effect with aureomycin.

The fact that there is only a comparatively small number of compounds with which the borrelia infection of mice can be successfully treated is one of the reasons why this infection is consistently included in the general screening schedule of our laboratory. In the course of the routine screening procedure, the anti-borrelia activity of borrelidin was found.

Before discussing the observations on the effect of borrelidin in the borrelia infection, it might be appropriate to describe briefly the other biological properties of this compound, particularly its toxicity.

TABLE 1 is meant to show that borrelidin is a comparatively toxic compound. Mice and young chicks were somewhat more resistant to the toxic effect of the antibiotic, but rats and rabbits were highly sensitive to it and so were humans. The main symptom of the acute poisoning was a hemorrhagic enteritis. Repeated administration of borrelidin required a considerable reduction of the dose. Even the more resistant mice and chicks did not tolerate more than 1.5 mg/kg over periods of 15–21 days. In dogs, the dose had to be reduced to 0.2 mg/kg and less.

It was surprising that even fractions of the small doses, which could be administered by the subcutaneous route, exerted a marked activity in the borrelia infection of mice. This is shown by the two examples in TABLE 2 and TABLE 3.

TABLE 1
ACUTE TOXICITY OF SINGLE DOSES OF BORRELIDIN

Animal	*Weight*	*Route of administration*	*LD_{50} mg/kg*
Mouse	16 – 20 g	subcut.	74.7
		intraven.	39.0
Rat	80 –120 g	subcut.	1.78
Rabbit	2.5– 3.0 kg	intraven.	2.37
Chick	250 –300 g	intramusc.	>4.2

The tables present the results of the subcutaneous treatment with borrelidin administered 20–24 hours after an intra-abdominal infection. At that time, a manifest infection of the blood had developed. The data are based on daily examinations of the blood. In the later stages of the observation, the examination was occasionally omitted on a Sunday. The examples show that doses of 1.5 mg/kg in case of the very sensitive strain *B.* tick and 3.0 mg/kg in case of *B. Novyi* cured all mice and sterilized the brain. Doses of 0.75 mg/kg and 1.5 mg/kg, respectively, cleared the peripheral blood only temporarily, and relapses occurred in the majority of cases.

TABLE 4 gives a synopsis of our experiments on three strains of borrelia and indicates that all strains responded well to the treatment. The ratio LD50/CD50 was 125, 37, and 44 for *B.* tick, *B. Novyi*, and *B. Obermeieri*, respectively.

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* Research Laboratories, Hoffman-La Roche Inc., Nutley, N. J. This paper, illustrated with lantern slides, was one of a group presented on the subject, *Borrelidin—A New Antibiotic*, at the meeting of the Division on March 25, 1949.

There was no definite influence of different dosage schedules on the effect of borrelidin. The data on the table are mostly based on a single subcutaneous administration of the antibiotic. Subdivision of the active doses into 2 to 4 treatments did not materially change the results, but, if the total dose was divided into 10 treatments with very small doses, a markedly lower effect was observed.

According to Berger's experience, borrelidin-like substances can also be demonstrated in concentrates from other sources than *Streptomyces Rochei*, for instance, from impure penicillin and from Clarase, an enzyme mixture used for inactivation of penicillin. These concentrates, which microbiologically appeared closely related to borrelidin, also possessed its anti-borrelia effect. This is shown in TABLE 5.

In this table the doses are given in units, because the weight of active material could

TABLE 2

ACTIVITY OF BORRELIDIN IN *Borrelia* (TICK STRAIN)

Mouse no.	*Parasites in blood**	*Borrelidin† mg/kg*	*Days after treatment* 1–4	5	6	8	9–16	16–24
1	+		0	0	0	0	0	0
2	+		0	0	0	0	0	0
3	+	1.5	0	0	0	0	0	0
4	+		0	0	0	0	0	0
5	+		0	0	0	0	0	0
6	+		0	0	0	0	0	0
7	+		0	0	0	+		
8	+		0	0	0	0	0	0
9	+	0.75	0	0	0	0	0	0
10	+		0	0	+			
11	+		0	0	+			
12	+		0	+				
13	+		+++					
14	+		+++					
15	+	Controls	+++	Dead				
16	+		+++					

* Infection: ∾ 600,000 parasites intra-abdominally.
† Treatment 1 day after infection; single subcutaneous treatment.

TABLE 3

ACTIVITY OF BORRELIDIN IN *Borrelia Novyi*

Mouse no.	*Parasites in blood**	*Borrelidin† mg/kg*	*Days after treatment* 1	2	5	7	9	16	20	*Brain transfer*
1	+‡		0	0	0	0	0	0	0	
2	+		0	0	0	0	0	+		
3	+	3.0	0	0	0	0	0	0	0	Negative
4	+		0	0	0	0	0	0	0	
5	+		0	0	0	0	0	0	0	
6	+		0	0	0	0	0	0	0	
7	+		0	0	0	0	0	0	0	
8	+		0	0	0	0	+			
9	+		0	0	0	0	0	0	0	
10	+		0	0	++					
11	+	1.5	0	0	0	0	+			
12	+		0	0	0	0	0	0	0	
13	+		0	0	0	0	0	0	0	
14	+		0	0	0	0	+			
15	+		0	0	0	+				
16	+		+++	Dead						
17	+		+++	Dead						
18	+		+++	Dead						

* Infection: ∾ 800,000 parasites intra-abdominally.
† Treatment· One day after infection; single subcutaneous injection.
‡ ∾ 650 per 100 fields.

not be accurately determined by weight in the concentrates. Nevertheless, the comparison with crystalline borrelidin makes it quite evident that the activity of the substance contained in the Clarase was quantitatively identical with that of the pure antibiotic. Similar results were obtained with material from impure penicillin.

It was mentioned in the beginning of this paper that comparatively few compounds or groups of compounds show an effect in borrelia infections. It seemed of interest to determine whether the activity of borrelidin was biologically related to any of the known agents. The most suitable method for such an attempt is the use of a specifically drug-resistant strain.

TABLE 4

EFFECT OF BORRELIDIN IN EXPERIMENTAL INFECTIONS OF MICE WITH 3 DIFFERENT STRAINS OF *Borrelia*

*Strain Borrelia**	*Curative dose mg./kg.*	*Relapse dose mg./kg.*	*CD_{50} mg /kg.*
Tick	0.815	0.415	0.6
Novyi	3.25	1.06	2.0
Obermeieri	3.6	0.45	1.7

* Intra-abdominal infection with 400,000–500,000 parasites. Subcutaneous treatment.

In case of borrelia, this is not too easy a task. To our knowledge, no high degree of drug fastness toward any of the known drugs was ever achieved in borrelia, despite the efforts of many good and experienced investigators. Also, we were not very successful in our attempts to prepare a penicillin resistant strain of *B. Novyi.* A temporary reduction of penicillin sensitivity was obtained, however, and it could be shown that this strain responded normally to borrelidin.

It was fortunate that it was much easier to render *B. Obermeieri* resistant to borrelidin. Starting with an inactive dose of 1 mg/kg, a borrelidin fast strain was obtained in 40 serial passages with increasing doses, during which the strain became resistant to 30 mg/kg. This resistance was strictly specific, as is shown in TABLE 6.

The table is self-explanatory. All of the 4 other anti-borrelia agents are as active in the infection with the borrelidin fast strain as they are on the parent strain, thus indicating that borrelidin is a representative of another class of anti-borrelia agents.

We should also like to stress the point that the activity of borrelidin is limited to the anti-borrelia effect. It is a chemotherapeutic agent of particularly high specificity. Trypanosomal infections were not influenced by the antibiotic, and rabbit syphilis did not respond even to toxic doses. This was a very surprising observation, because, to our present knowledge, all substances which are active in borrelia infections also exert an effect on *Treponema pallidum.*

TABLE 5

COMPARISON OF BORRELIDIN FROM *Streptomyces Rochei* AND FROM CLARASE IN *Borrelia Obermeieri* INFECTION OF MICE

*Compound from**	*Dose u./kg.*	*No. of mice*	*Number cured*	*Number relapsed*	*Number no effect*
Streptomyces Rochei	37,500†	6	4	2	0
Clarase	38,000	6	5	1	0
Clarase	19,000	6	2	4	0
Clarase	8,500	6	0	0	6

* Therapeutic experiment; single subcutaneous treatment.
† = 2.25 mg./kg

TABLE 6

COMPARISON OF THE ACTIVITY OF MAPHARSEN, MYOCHRYSIN, PENICILLIN G, AND BORRELIDIN ON THE PARENT STRAIN OF *B. Obermeieri* AND ITS BORRELIDIN FAST MODIFICATION (BR-STRAIN)

Substance tested	*Active group*	*Minimal active dose: mg./kg. Parent Strain*	*Minimal active dose: mg./kg. BR Strain*
Mapharsen	As	6.25	6.25
Myochrysin	Au	25–50.00	50.00
Penicillin G	—	60.0*	60.0
Aureomycin	—	12.5	12.5
Borrelidin	—	1.5	30.0 (inactive)

* 4 treatments of 15 mg./kg.

There was, however, one interesting property of borrelidin with regard to the *T. pallidum* infection of rabbits, and this was the enhancement of the anti-syphilitic effect of penicillin G. It is known from the work of Rake and his co-workers[5] that impure penicillin showed a higher activity in rabbit syphilis than the crystalline material. As borrelidin might be one of the impurities present in crude penicillin, experiments were carried out by Soo-Hoo to study the influence of small doses of borrelidin added to crystalline penicillin G. The technique used was the determination of the protective dose of penicillin against an intradermal infection with *T. pallidum*, as recently suggested by Eagle, Magnusson, and Fleischman[6] and by Turner, Cumberland, and Li.[7] The rabbits were infected intradermally with 40,000 spirochetes and received a single intramuscular injection of penicillin or penicillin + borrelidin in an oil-beeswax mixture 24 hours after the intradermal infection.

There was a definite trend to increased activity if a small amount of borrelidin of 0.005 mg/kg was added to the crystalline penicillin G. The CD50 of crystalline penicillin G was 1.3 mg/kg; in presence of borrelidin, 1.2 mg/kg still protected the majority of animals (75%) and even 0.6 mg/kg was active.

Summary. Summarizing the experimental findings presented in this paper, it appears that borrelidin is a potent antibiotic with specific anti-borrelia activity. This effect was obtained with doses far below the toxic level of the substance. Although borrelidin was found to be without effect in experimental rabbit syphilis, there was a trend to enhancement of the anti-syphilitic action of crystalline penicillin G if borrelidin was added in small quantities.

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ENHANCEMENT OF PENICILLIN ACTIVITY BY BORRELIDIN

E. GRUNBERG, D ELDRIDGE, G SOO-HOO, AND D R. KELLY[*]

Dr. Goldberg and his associates[1, 2, 2a] have pointed out the reasons which made them expect that borrelidin might be one of the factors responsible for the enhanced activity of penicillin G. One example, the enhancement of the anti-syphilitic action of crystalline penicillin G, has already been given.[3]

From the literature, it is known that increased activity of crystalline penicillin G by impurities has been observed in a number of experimental infections, even though no convincing clinical evidence of superior activity of impure penicillin is available. It might be appropriate to summarize the investigations that have already been carried out by different authors on the enhancement factors of penicillin G. These data may be observed in TABLE 1. Dunham and Rake[4] were the first authors to report an enhanced effect of penicillin G in the experimental treatment of rabbit syphilis. This was followed shortly by the work of Hobby *et al.*,[5] who showed the presence of enhancement factors of penicillin G in the experimental treatment of streptococcal and pneumococcal infection, by Welch and coworkers[6] in typhoid infections of mice, by Miller *et al.*[7] in the endotoxin poisoning of mice, by Cole[8] in the *in vitro* inhibition of trichomonas, and by Burk *et al.*,[9] who observed an enhancement effect of penicillin G in the inhibition of respiration of staphylococcus 209.

Before discussing borrelidin as a possible enhancement factor of penicillin G, it should be noted that the borrelia infection of mice was the only experimental model which responded to borrelidin. Included among the tests which showed no response to borrelidin *per se* are infections with β-hemolytic streptococci, pneumococci type 1 and 2, *Klebsiella pneumoniae*, *Salmonella schottmuelleri*, *Eberthella typhi*, *Mycobacterium tu-*

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[*] Research Laboratories, Hoffman-La Roche Inc., Nutley, N. J. This paper, illustrated with lantern slides, was one of a group presented on the subject, *Borrelidin—A New Antibiotic*, at the meeting of the Division on March 25, 1949.

berculosis (*H 37 Rv*), influenza type A virus (PR8-strain), murine poliomyelitis virus (SK strain), rabbit syphilis (Nichols strain), *T. equiperdum* and *Cryptococcus hominis.*

A considerable amount of work was involved in our investigations in an attempt to prove the enhancement effect of borrelidin in the experimental models which have been enumerated in TABLE 1. We are now going to present the results of our experimental work concerning the study of the possible enhancement of penicillin G with borrelidin. We would like, first, to present all instances in which an enhancement activity of penicillin G was observed.

TABLE 1

COMPARISON OF PROPERTIES OF VARIOUS PENICILLIN ENHANCEMENT FACTORS

Author	*Typhoid*	*Streptococcus*	*Pneumococcus*	*Syphilis*	*Endotoxins*	*Respiration*	*Trichomonas*
Welch	X					X	
Hobby		X	X				
Dunham and Rake				X			
Miller					X		
Burk	—					X	
Cole							X
Borrelidin	—	X	—	(X)	(X)	X	X

(X) = trend towards enhancement.
X = Enhancement effect or inhibition of respiration of Staph. 209 (Burk).
— = No enhancement effect
Blank = No results available

A survey of our work with penicillin in streptococcal infections carried out during the last five years showed that the activity of penicillin G has remained practically the same since the time when the first batches of impure penicillin containing 100 units/mg. were tested in streptococcal infections of mice. The subcutaneous CD50 in those early experiments and in those from 1943 to the present time was between 300 and 500 units/kg. There was, however, one important difference. The less pure penicillin samples frequently maintained this CD50 with a single treatment, while the more recent and purer samples, which gave the same CD50, refer to two treatments with one-half this dose. The same effect of a single treatment with crystalline penicillin G required 3 to 4 times higher doses, the CD50 being between 1000 and 1500 units/kg.

Studies were, therefore, begun on the influence of borrelidin on the anti-streptococcal activity of crystalline penicillin G by using the single treatment method. These experiments were carried out with *Streptococcus hemolyticus C 203.* The infective dose was 0.5 ml. of a 10^{-5} dilution representing approximately 1000 MLD. A single subcutaneous treatment was given shortly after infection. Succumbing mice were autopsied and the blood cultured. Surviving animals were observed for 21 days, since, as is well-known, late death from streptococcus infection might still occur after 10 or 15 days and no experiment can be considered stabilized until 20 days have elapsed.

Needless to say, a very large number of animals was used before the optimal range of relationship of penicillin and borrelidin doses was found, even though suitable dosages of penicillin alone were known from previous experiments.

In TABLE 2 are the results of those small doses of borrelidin which, when combined with doses of penicillin giving only 40 per cent survival, increased the survival rate of mice infected with streptotocci. In this table, the results of a single subcutaneous treatment are given. There is a certain variation as to the optimal dose required to give the enhancement effect. It seems that the smaller the penicillin dose, the smaller will also be the optimal borrelidin dose.

If the CD50 of penicillin is calculated by the cumulative method of Reed and Muench[10] for all the groups of mice treated by a single subcutaneous injection, with and without borrelidin, a significant difference is evident. The penicillin CD50 calculated on this basis is 0.56 mg./kg. for penicillin G alone and 0.25 mg./kg. for penicillin and borrelidin combined. It is interesting to note that this figure, 0.25 mg./kg. or 420 units/kg., is very close to the figure evaluated many years ago with impure penicillin. The material used in 1942–1944 was a sample combined from several experimental batches of a potency of 100 units/mg. In a streptococcal infection treated by a single subcutaneous injection, a CD50 of 300 units/kg. was obtained. These data mean, therefore, that the addition of borrelidin restituted the original activity of less pure penicillin which had decreased in more highly purified and crystalline products of penicillin G.

TABLE 2

ENHANCEMENT OF THE ACTIVITY OF PENICILLIN G BY THE ADDITION OF BORRELIDIN IN STREPTOCOCCAL INFECTIONS IN MICE

Penicillin		*Per cent Survivors*					
		Borrelidin					
units/ kg.	*mg / kg.*	*0* *0*	*3.125* *0.0002*	*6.25* *0.0004*	*12.5* *0.0008*	*25.0* *0.0017*	*units/kg.* *mg./kg.*
1500	0.9	40	—	60	100	79	
500	0.3	41.5	70	55	—	—	
250	0.15	40	—	60	—	—	

Controls: 1%.
Infection: 0.5 cc of 10^{-6} dilution intraperitoneally (Strain C203).
Treatment: 1 × subcutaneously.

TABLE 3

THE EFFECT OF PENICILLIN G AND PENICILLIN G PLUS BORRELIDIN ON STREPTOCOCCAL INFECTIONS *In Vivo*

Penicillin		*Borrelidin*				
units/kg.	*mg./kg.*	*0* *0*	*2.5* *0.00017*	*5.0* *0.00033*	*10* *0.00067*	*units/kg.* *mg./kg.*
125	0.075	8/30 =26.7%	10/30 =33%	15/30 =50%	11/30 =37%	

Controls: 0/30
Infection: 0 5 cc. of 10^{-5} dilution intraperitoneally (Strain: Strep. 4)
Treatment: 2 × subcutaneously

We would like to emphasize the fact that the enhancement effect of penicillin G by borrelidin is only demonstrable if borderline doses of activity are used. Fully active and completely inactive dosages do not show the influence of borrelidin. This influence with borderline doses was statistically analyzed and a χ square value was obtained, corresponding to a probability (p) of considerably less than 0.001.

Since the CD50 of 420 units/kg. of penicillin plus borrelidin which was observed in the single treatment of a streptococcal infection is very close to the CD50 for treatment with 2 injections of 187.5 units/kg., which gives a final total of 375 units/kg., the question arose as to whether borrelidin also increased the activity of penicillin G given in 2 injections. This was indeed the case. In the experiment recorded in TABLE 3, which was carried out with streptococcus 4, 2 treatments with penicillin, 0.075 mg./kg., were given at four-hour intervals. This dose, which, as you can see, is well below the CD50 of penicillinG, alone gave 50 per cent survival if combined with 0.33 µg./kg. borrelidin. A statistical analysis of the figures has shown them also to be significant.

A number of experiments were also devoted to the question of enhancement of orally-administered penicillin in streptococcal infections. Here, as was observed in the subcutaneous treatment, small doses of borrelidin produced a marked increase of activity of penicillin G. The CD50 of penicillin G and 3 µg. of borrelidin per kilogram was 0.7 mg./kg., in contrast to 1.16 mg./kg. of penicillin alone.

Before passing on to any discussion of other infections, we would like to point out that high doses of borrelidin interfered with the activity of penicillin which seemed to us to be similar to the hormetic effect described by Welch.[11] The majority of mice undergoing combined treatment by the subcutaneous as well as by the oral route died of the streptococcus infection if the ratio of penicillin to borrelidin was 1 to 0.3 or higher.

Another experimental model in which the enhancement effect of penicillin G by borrelidin was observed was the meningococcal endotoxin poisoning of mice. Meningococcus endotoxin was prepared according to the method of Boor and Miller[12] and the animals received an intra-abdominal injection of from 0.4 to 0.6 ml. Two subcutaneous treatments were given before the injection of toxin and two more after the toxin injection, at intervals of two hours. The results are given in TABLE 4. PR8 as given in this table were old batches of impure penicillin containing 300–400 units/mg. This table shows that impure penicillin, PR8, was considerably more active than penicillin G, thus confirming the observation of Miller and his group.[7] The addition of a small dose of borrelidin also increased the activity, but to a lesser degree than that of PR8.

TABLE 4

ACTIVITY OF CRUDE PENICILLIN PR8, CRYSTALLINE PENICILLIN G, AND PENICILLIN G PLUS BORRELIDIN IN THE MENINGOCOCCUS ENDOTOXIN POISONING OF MICE

Drug	*Dose mg./kg.*	*No of mice*	*Survivors No.*	*Survivors %*
PR8	4 × 50,000	30	16	53.0
Penicillin G	4 × 50,000	30	2	6.6
Penicillin G + Borrelidin	4 × 50.000 + 4 × 16.1 μg.	50	13	26.0
Controls	—	70	3	4.3

Endotoxin dose: 0.4–0 6 ml.

Treatment: 3–4 times, 2 treatments before, 2 after the endotoxin dose. Subcutaneous administration.

So far, this paper has discussed *in vivo* infections. At this point, a digression will be made to describe the only *in vitro* experiment in which an enhancement of activity of penicillin G by borrelidin was observed. We were able to confirm the findings of Cole,[8] namely, that impure amorphous penicillin exerted an anti-trichomonas activity at 2500 units/ml., while crystalline penicillin G was ineffective in concentrations greater than 10,000 units/ml. The addition of suitable concentrations of borrelidin, 0.0005–0.0007 mg./ml., which showed no appreciable growth inhibiting effect on trichomonas, resulted in an active dosage range of penicillin 1250–2500 units/ml., which equaled or surpassed the dosage range of impure penicillin.

Returning now to the *in vivo* models, experiments were also carried out with *Diplococcus pneumoniae* in which small doses of borrelidin were combined with various doses of penicillin G. The range of total doses of penicillin tested, as can be seen from TABLE 5, was between 0.3 mg./kg., which is an inactive dose, and 1.8 mg./kg., which cured 90 per cent of the animals. The results may also be seen in TABLE 5. This table shows that the lowest penicillin dose was not changed by borrelidin, and the same is true for the highest dose. The doses of borrelidin still seem quite high and should be titrated further. The variations, which are unavoidable, have been found to be insignificant.

TABLE 5

COMBINATION OF PENICILLIN G AND BORRELIDIN IN PNEUMOCOCCAL INFECTIONS OF MICE

Penicillin mg./kg.	*Borrelidin mg./kg.*				
	0	*0.3–0.5*	*0.75–1.0*	*1.5–2.0*	*3.0*
0.3	0/10	—	—	0/10	—
0.6	7/30[(a)]	0/10	0/10	6/20	—
0.9	31/50	5/20	17/30	25/40	4/20
1.8	18/20	9/10	10/10	—	—

Controls. 0/80[(a)]

(a) 7/30 = survivors out of 30 mice
0/80 = survivors out of 80 mice

Infection: 0 3 cc 10^{-6} dilution intraperitoneally

Treatment: Subcutaneously.

TABLE 6

INFLUENCE OF BORRELIDIN ON THE ANTI-TYPHOID ACTIVITY OF PENICILLIN G

Strain	*Penicillin mg./kg.*	*Average percentage of survivors Borrelidin mg./kg.*			
		0	*0.003–0.03*	*0.17*	*0.3–2.0*
PC1–412	90	85	55	65	—
	60	52	53	6.6	—
	45	70	20	—	—
P58A	90	65	63	50.0	—
	75	40	10	—	—
	60	14	3	—	25
P58A	60	70	60	—	47.5
(mucin)	30	60	45	—	27.5

The same type of results was obtained with oral treatment of this infection.

Welch's studies on the enhancement effect in typhoid infections were of particular interest. Experiments were carried out with two techniques using the well-known P58 A strain of *Eberthella typhi* and with a strain that was kindly furnished us by Dr. Welch, PCI-412. Two different types of experiments were carried out using strain P58 A. One, similar to the technique used by Welch, was the injection of a large amount of *E. typhi*, namely, 0.4 ml. of a six-hour broth culture, while the other was the type of infection usually used in our laboratories, the injection of 0.5 ml. of a 10^{-6} dilution of an overnight broth culture suspended in mucin. The infection with strain PCI-412 was that used by Welch and his co-workers.

As can be seen from TABLE 6, no enhancement effect was observed. In fact, doses which show a higher ratio of borrelidin to penicillin seem to interfere to a certain degree with penicillin G.

The final experimental model to be presented here is the experiment with *Salmonella schottmuelleri* as shown in TABLE 7. Here, once again, no enhancement of penicillin G by borrelidin is observed.

In conclusion, it might be of interest to you to summarize the properties of enhancement of penicillin G by borrelidin and the enhancement factors investigated by different authors. This is done in TABLE 1. This table shows that borrelidin exhibits an enhancement of crystalline penicillin G, or at least a trend toward enhancement, in five of the seven experiments listed. It had no influence on *Eberthella typhi* or *Diplococcus pneumoniae* infections of mice if combined with penicillin G. Since it is not known whether the previously investigated enhancement factors could display other properties than those discussed in the literature, it is not possible to decide whether borrelidin is or is not identical with any of them. Furthermore, borrelidin is a pure compound identifiable by at least two specific biological properties, the *in vitro* effect on *Sarcina lutea* and the antiborrelia effect *in vivo*. No information is available to us at the present moment as to similar properties of other factors.

TABLE 7

ACTIVITY OF PENICILLIN G WITH AND WITHOUT BORRELIDIN. EXPERIMENTAL INFECTIONS WITH *Salmonella schottmuelleri*

Penicillin G mg /kg.	*Borrelidin mg./kg.* 0	0.033	0.066	0.132
120	8/10	8/10	10/10	—
66	16/30	10/20	20/30	3/10
45	3/10	1/10	3/10	2/10
30	1/10	—	0/10	—

Controls: 0/50.
Infection: 0.5 cc. 10^{-7} dilution of broth culture.
Treatment: 1 × subcutaneously.

Summary. Borrelidin was found to exhibit an enhancement effect on penicillin G in experimental infections with beta-hemolytic streptococci and meningococcus endotoxin. This influence seemed to be dependent on the dose of penicillin as well as on the borrelidin dose when a combination was administered by the parenteral or oral route. Very high doses of borrelidin seemed to interfere with the activity of penicillin. No enhancement effect of borrelidin was observed in infections with *Diplococcus pneumoniae*, *Eberthella typhi*, or *Salmonella schottmuelleri*.

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SECTION OF PSYCHOLOGY

THE Q-TECHNIQUE STUDY OF PERSONALITY

By WILLIAM STEPHENSON*

The study of personality seems, like Gaul, to be divided into three parts. One is concerned with literary and metaphysical or philosophical descriptions and analyses. Another is called, by some, *clinical* psychology. To the third, is given the name factorial analyses. Novelists are usually eminent in the first region, although some psychoanalysts do as well or better. Amongst many others, the standpoint of Gordon Allport,[1] with its *idiographic* trend, fits well into the second region. The early work of the Spearman School in London, with more recent excursions by Sir C. Burt,[2] his assistant Dr. Banks,[3] and Professor R. B. Cattell,[4] are, of course, examples of the third. The present paper begins a process of looking behind all three regions to study certain logical identities. Everyone knows, for example, that factorial analysis is rooted in statistical theorems. Few, however, would say that *les chiffres* count for much in clinical psychology, where insight is supposed to take their place. Amongst novelists, of course, the preoccupation with numbers is merely mercenary. Yet, I think it can be shown that, whereas the factorial analyst used numbers explicitly and objectively, the clinicians and novelists use them only implicitly and subjectively, although their thinking involves the selfsame procedure. In any case, it is certain that what the novelists and idiographists think about personality can be represented in statistical terms with considerable verisimilitude, if not precision, and that the ordinary methods of factorial analysis can be applied to these representations.

That this is the case was shown first, I think, in the application of Q-technique to Jung's great essay on *Psychological Types*,[5] which is a mixture of literary and clinical regards. Previous systematic study of introversion-extraversion had been in terms of individual differences, with disappointing results, whereas, what was required as a beginning was merely the direct representation of personalities to determine their similarities. Jung made use of some 2,000 traits to describe the introversion and extraversion features of personality. In Q-technique, these are defined as a *trait-universe*, from which samples may be drawn at random. Any personality can then be represented in terms of the sample, by giving high scores for traits which characterize it, and low for those which do not do so. If we assume that, for such a random sample, the *mean* score for the sample of traits will not differ much from personality to personality, and that the scores given to the large sample of traits will tend to be normally distributed, the conditions at once exist for the reduction of the data to standard scores, and for applications of the product-moment correlation coefficient. Different personalities can be correlated in terms of the sample of traits, and sets of such correlations can be factorized. I have already reported several brief studies of Jung typology carried out in such terms.[6, 7] It is sufficient to say that any of Jung's hypotheses can now be tested in a direct scientific manner, and that this was never achieved in terms of R-technique studies, that is, by way of the study of individual differences for samples of *persons* drawn from any defined population or universe of persons.

Similarly, it is possible to take a book of clinical case studies, such as Beck's[8] descriptions of personalities tested by the Rorschach method, and to reduce the literary compilations of details to a trait-universe of Rorschachian terms. Again, this universe can be sampled and personalities described in terms of the sample or samples, and these can be correlated. Some hundreds of pages of descriptions can be reduced, in this way, to the compass of a single correlation table, with as many variables in it as there are cases in Beck's book.

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Shorthand statistical descriptions of this kind, however, are merely a beginning. Its consequence is that hypotheses can be stated, tested, and proved or disproved in scientific fashion. I have already described a number of studies of this kind in Q-technique. In one, for example, two trait universes were defined, one consisting of 800 everyday terms that I found myself using in giving accounts of personalities to business executives in language which they could understand, and another a universe of 500 Rorschachian terms, taken from the aforementioned book of Beck's.[8] Samples of 400 were drawn from the former at random, and 120 from the latter. Ten men were then represented in such a way as to afford what I believe to be the first systematic attempt to offer some proof or disproof of Allport's hypothesis of functional autonomy.[1] For it was argued that, if for these men working with Rorschach traits of a presumably fundamental or underlying nature, the same factor patterns result for descriptions by the psychologist, as those by a number of naive subjects working with the everyday language traits, then by so much is functional autonomy *not* proven. Evidence pointed to the latter conclusion, of course only with reference to the Rorschach test conditions. The main point, however, is not that I want to argue for or against such a hypothesis at present. It is merely a reminder that a technique now exists for putting hypotheses such as Allport's to crucial tests.

In a general way, too, it is worth noting that different trait-universes may be held to represent different psychological or behavioral field conditions.[7] Thus, the language we use to describe personality to business men is quite different from that we would employ in reporting about nurses to a matron. We implicitly couch our terms to suit the circumstances. It is now possible, therefore, to study people in different circumstances by factor methods: the business man at work, at play, at home, with the appropriate trait-universes involved in each case.

But, on the same grounds, systematic *typology* may be said to have its roots in this same technique. Chein,[9] for example, like many others, argues that what is really involved in personality is the search for "enduring or persistent motives" which account for the intransigence of traits in different field conditions. The aforementioned Rorschach study had such a basis to it: that of more fundamental or persistent traits, which underlie the everyday descriptions we offer of a personality. In fact, typology, in general, can be conceived as systems of trait universes, one above another, with the more fundamental ones below.[7]

Again, the preoccupation of Guttman[10] and others in the search for "homogeneous" or "pure" tests and the like is psychometrically based in this same technique, as Loevinger[11] is perhaps noticing.

Finally, by way of preamble, Q-technique can be applied to mere handfuls of persons. Indeed, if need be, only *one* person could be studied, and large tables of correlations provided about him. There is nothing unusual in this. But an important possibility exists that factors determined for a few persons only, as few as half-a-dozen persons perhaps, can provide all the evidence one requires to prove or disprove psychological hypotheses, or to indicate what is *necessarily* and *alone* possible, even if hundreds of persons were tested instead. This, and the attendant practical facility of being able to apply factor devices with great precision to one or a few persons only, mark out the technique as a tool *par excellence* for clinical studies.

These various applications, and the assertions they involve, follow from the fact that, whereas earlier work had its *systematic* foundations laid in the definition of universes of persons, which could be sampled so that individual differences were adumbrated, Q-technique works, instead, with a far more available and amenable domain of universes, namely, those of traits or other particularities. These, as I have shown, can be handled systematically, just as can be universes of people; and the traits can be quantified with respect to many current personality concepts, such as "characteristicalness" of a trait in a personality, "idealisation," "projection," "rationalization," and the like.[6] But when a sample of traits is so quantified, as I have said, the *mean* score for all persons is the same. There are no individual differences in this regard. Thus, I find that I can reasonably represent the personalities of university students in terms of a sample of 210 traits drawn from Cattell's enquiries,[4] in such a way that their means are the same for all students, and their standard deviations too. Students can do the same for themselves just as readily. This can be done for the simple reason that there are as many statements of a positive nature in the trait-universe, or in the sample, as there are negative ones. For an item of the kind "trustworthy," there is always another such as "disloyal." If the one is given a high mark in characterizing a personality, the other receives a low one. In short, the means for significance for a large enough sample of traits will tend to be the same for all persons. For systematic purposes, we reduce the standard deviations of such arrays, about each person, to standard terms. That is, the product-moment correlation coefficient is at once applicable to the data without in any way doing violence to it. If I seek to represent extreme personalities, however, such as that of a severely hysterical-depressive woman, the condition that the *means* are zero is still true, but wider dispersions may be required in order to do justice to the case.

The matter I have just referred to has some unexpected ramifications. In R-technique analysis of mental tests, the correlational approach is very pertinent, because the *means* or *averages* provided by tests, for a sample of persons, are in a way quite arbitrary matters. If any test is made longer, or shorter, its mean would ordinarily alter *pari passu.* Factorists, therefore, correlate such tests, and this, of course, is based upon standard scores, *i.e.*, the mean of any test is made 0, and its standard deviation 1.0. As I say, this is a wholly reasonable and sensible procedure. But the same does not necessarily apply to *traits* which are quantified with respect to individual differences, for now we can't, so to say, arbitrarily alter the length of a trait. Thus, when Burt and Cattell take separate traits, such as . . . "assiduous," "interested in persons of the opposite sex," *etc.*, and quantify these with respect to individual differences for assumed randomized conditions, it is a considerable assumption to suppose that, as a *first* approximation, the *means* in these cases must be 0, and the standard deviations 1.0. On the contrary, the first assumption is likely to be that the *means* will be *necessarily* vastly different. One merely has to ask 100 women to estimate (1) their assiduousness, (2) their interest in men, on a 10-point scale, to see that (1) gains about 5 points on the average, and (2), 9 or 10. If, then, one were to seek to study these traits at a grass root level, it is gratuitous to assume that these means should be reduced to 0 in each case, whatever one might say about their standard deviations. There is nothing logically necessary in the assumption, as there is for mental tests. Yet Cattell and Burt go ahead without a moment's pause to wonder whether this assumption is warranted. Burt, it is true, would say that a Q-technique approach to such traits is preferable,[2] but he makes no suggestion as to why this is so.

It may well be that in neglecting the differences in *means* between traits, as Burt and Cattell do, the very essence of matters is thrown away, like a baby with the bathwater. There is some evidence already, indeed, that this is the case. It comes from a study I have made recently of Professor R. B. Cattell's list of 35 conative traits, such as "suspicious," "anxious," "frank." These had been studied originally by Cattell as a matter of R-technique, and he has provided a table of correlations for these traits as variables, for a sample population of 230 women. Factorized by Thurstone procedures, some 18 factors were presumed to be isolated by Cattell and identified after 20 months of rotation and study. Dr. Banks[3] has recently refactored the same table, using

Burt's summation procedures, and, after a month of work, provides a different set of factors, involving in her case, with nice crystallographic exactness, one general factor, a bipolar whose parts, divided exactly in half, provide another bipolar, and each of these halves, divided in half again into eight smaller parts. This geometrical patterning corresponds, also, to basic and group factors with which many of us have become more familiar, namely: a general or "basic" factor of *emotional stability or instability* (spiteful, self-willed, attention-getting, *etc.*); a group or broad factor of "cheerfulness" (the euthymic qualities, talkative, sociable, cheerful, *etc.*); another group factor identified with *esthenic-asthenic* qualities (roughly extraverted or introverted traits, such as immature, fickle, boorish, frivolous) and eight smaller or "narrower" factors, identified as *paranoid*, *compulsive*, *anxiety*, *adolescent hysteria*, *dominating*, *impulsive*, *cordial*, and *facile*, respectively. Banks considers that these factors are structurally related, whereas Cattell's are not so. But this seems only to mean that a hierarchy of classes is involved, with a *summum genus*, subordinate genera, narrower species, and still narrower sub-species.

Now it happens that I have also re-factored the same Cattell table, using Spearman methods, and arrive, after three days of work, at four group factors (ignoring some details, much as did Banks in her analysis, since they are not material to the present argument). These four factors give a reasonably excellent fit. They correspond very approximately to Banks' factors I, VII, and III (for my first three factors respectively), but my fourth factor has no obvious counterpart in her table. My interpretation of the factors, however, is very different.

This follows from two or three considerations, and from the Q-technique study I have made of the same traits. First, however, we should note that, whereas, Cattell's analysis is rightly enough empirical, nothing in it suggests that the women might be subject to rationalizations, stereotypies, and the like disturbers of orderly self-appraisals. Similarly in Banks' study, nothing of the kind is considered or suspected. It may be, of course, that these effects are thrown away with the bath-water, *means* which are, as I have indicated, basic to R-technique. And, if this is the case, one might well say good riddance to them, especially, if what is left behind has the precision and nicety of the hierarchy of factors provided by Banks. It is with this matter in mind that I turned to a Q-technique study of these same traits.

Professor Cattell kindly provided me with a detailed list of the 35 traits, each of which is there described in short paragraphs which subsume a number of apparently comparable terms. Thus, "depressed, solemn," is described as . . . "earnest and solemn most of the time. Not easily moved to laughter. Seeming slow and depressed rather frequently." This short paragraph contains at least seven separable statements, as do each of the others more or less. It was, thus, possible to compose a trait-universe of some 600 items from Cattell's list. A sample of 210 was taken. These were printed on cards, which the subjects could handle in three sets of 70 at a time.

In one study, 17 college women were employed. They were told to represent their own personalities, in a general context, in terms of each set of cards, on a prearranged frequency distribution.[5] Meanwhile, I had clinically tested 12 of the women, and could form my own judgment about their temperamental qualities.* I could, therefore, represent each in turn on the same 210 traits, and so make possible the direct correlation of their self-appraisals with what I considered their personalities to be. In addition, I could add appraisals for four other women, all of university student type, with whom I have had a long acquaintanceship. One is my wife, and three others are friends my wife and I have had to handle over a period of years.

Several tables of correlations were put together. One contained the 17 students' own appraisals, plus mine of the four other women. Another contained the 12 students for whom I was able to make appraisals following the clinical testing to which I had subjected them. I found that the twelve students self-assessments correlated with my own assessment of them by amounts 0.47, 0.46, 0.43, 0.29, 0.28, 0.24, 0.18, 0.15, 0.13, 0.11, and 0.08 respectively. Only the first six of these are significant ($N = 210$, $SE = 0.07$ approximately). In the other six cases, the women have either no insight into their temperaments, or else are reflecting standard stereotypies, wittingly or otherwise. A shy, timid, dull, and inhibited woman, for example, assessed herself as above all else, "highly imaginative," "sophisticated—has wide interests and knowledge, especially intellectual." A most impulsive, almost a typical psychopathic student, regarded herself as "dependable and realistic—emotion doesn't get in the way of her judgment." There was patent and obvious lack of insight in these cases.

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* I made use of brief tests of G and K factors, *fluency*, Rorschach and Thematic Apperception, and *perseveration* tests

The appraisals upon which Cattell based his table, however, are not likely to involve such obvious crudities as these, since his were made by *other* students about their fellows in turn. Nevertheless, if, as a presumed expert, I cannot reach better agreement with the 12 women than the above would indicate, it is reasonable to wonder how much real insight students have in general about either their own, or other's, temperamental qualities. The suspicion becomes stronger when a Q-technique analysis is made of the tables of correlations for these students amongst each other as variables. For now, of four factors, in terms of which the correlations may be accounted, two are most reasonably interpreted as stereotyped notions of what women students would *like* to think they are, rather than an account of such matters as general stability of personality (Banks's and Burt's factor of general emotionality).

In short, there is considerable evidence of stereotypy in women's responses as soon as data of this kind are looked at by Q-technique procedures. Of four factors, for example, there was agreement between the women and myself in only one case. Considerable caution is needed, therefore, in the interpretation of factors issuing from R-technique, as has long been suspected.

I have to suggest, as a result of this and other studies, that there is nothing necessarily *basic* about the R-technique study of traits, since it throws away the most obvious information about them at the outset, namely, the differences in *means* (as a matter of individual differences). Q-technique does no violence of this order to the facts. Indeed, it leaves the facts untouched, except in so far as the assumption is made of equalized dispersions.

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SECTION OF ANTHROPOLOGY

THE CONCEPT OF LAND TENURE IN WORLD CULTURE

By JOHN M. COOPER*

Divergences between the thousand and one land tenure systems of the world are legion. But underneath the divergences are certain fundamental uniformities of dynamic, structure, and function, uniformities often masked by our divergent legal and anthropological terminologies. The present paper is an attempt to analyze and formulate some of the more important of these uniformities.

The land claims with which we are here concerned are claims to exclusive possession and occupation of a given area of the earth's surface, and to the exclusive exploitation and enjoyment of the area's resources and facilities. Where such exclusive rights of possession, occupation, and use are claimed by an integral autonomous political entity,—a band, tribe, nation, and so forth—as against alien entities, we have territorial sovereignty. Where they are claimed by individuals or by fractional groups—families, kinships, sibs, associations and so forth—within a given integral political entity, as against other individuals or fractional groups therein, we have land tenure.

Both territorial sovereignty and land tenure are found in practically all autonomous political entities—in all, if we interpret the terms leniently. Where, however, as occurs here and there, especially among some marginal peoples, a single conjugal family, or a very small group composed wholly or almost wholly of close kin, is, for all practical purposes, an autonomous political entity subordinate and beholden to no other such entity, it is merely a matter of words as to whether the land claims of such atomistic groups constitute territorial sovereignty or land tenure. In the strict sense of these terms, such a group would have territorial sovereignty, but not land tenure.

So much for definitions. We turn now to the uniformities. And first, all systems of land tenure, as regards both their uniformities and their divergences, are conditioned by and have their dynamic in the interplay of three sets of factors: psychological, social, and ecological.

The psychological factors are of both the conative and the cognitive order. Those of the conative order are, of course, the biological drives, particularly, but not solely, that of hunger, and the social ones, in so far as land-linked resources and facilities satisfy these desires. Tenure of land, as "exclusive," leaves the actual holder free to exploit the land without interference from others, and as "tenure," gives him security to hold it for a shorter or longer period. Psychological factors of the cognitive order are traditional or newly acquired techniques and skills for efficiently exploiting land-linked resources.

The social factors are the prevalent conceptions of self and of "one's own" for whom desire-fulfilment is wished. The conception of "one's own" will, of course, differ from one integral political entity to another, from one fractional group to another, and from individual to individual, depending, among other things, upon the solidarity, composition, and value-systems of the respective exploiters and holders.

The ecological factors are the land-linked resources and facilities recognized as capable, under use of known means, of satisfying the wants of the individual and of "his own." These land-linked resources and facilities comprise both tangible and intangible "goods." Among the more important land-linked tangible goods are: food, soil and surface for agricultural and pastoral production, clothing, light and fire, shelter, medicine, and manufactures. Among the more important land-linked intangible goods are: freedom of movement and transit, privacy, defence, recreation, esthetic enjoyment, prestige, power, sentimental attachments, "at-homeness," familiarity (important also for exploiting resources), religious loyalties and associations.

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These three sets of factors constitute the dynamic uniformity, it would seem, back of all systems of land tenure. This is the first uniformity, a fairly obvious one, to which attention is here given. To the second, that of selectiveness, we now turn.

Each culture, or each autonomous political entity or politically independent society of men living under a given culture, carries out in its land-tenure system a twofold selective process.

First, each such entity or society selects, in accordance with its specific desires and social organization, some resources and facilities for exploitation and enjoyment, and neglects others from among the almost numberless available tangible and intangible resources and facilities linked to the land it claims under territorial sovereignty. For example, a given non-literate people may thus neglect the mineral fuels under or on the surface of the land, whereas, to us Occidentals of the twentieth century, these resources may be the most valued land-linked goods in a given area. The divergence in this respect of selection between the land tenure systems of the world, as between our own Occidental ones and those of other peoples, is one, so to speak, not of kind, but of number and degree. Each system both selects and neglects. Our own modern American one, for example, differs from others, especially those of many non-literate ones, in selecting a greater number of resources and facilities and in neglecting fewer.

Second, from among the land-linked resources and facilities, tangible and intangible, actually selected for exploitation and enjoyment, each autonomous political entity reserves at least some for free non-exclusive exploitation and enjoyment by all its constituent individuals and fractional groups. Some entities reserve practically all, and then we have land tenure systems that are nearly 100 per cent communal. But no political entity, neither our own nor any other to our knowledge, at any level of technological achievement, turns over all its land-linked resources and facilities to its constituent individuals and fractional groups for their respective exclusive expolitation and enjoyment. In brief, in no political entity is the land tenure system 100 per cent private. Some systems have more of communal tenure, some more of private tenure, and these may be spoken of, respectively, as communally oriented or privately oriented. But the difference between the systems or between, for instance, our own system and those of non-literate peoples, is one of degree and proportion, rather than of kind. Between the most extreme private orientation, as in our own modern American system, and the most extreme communal one, as, for instance, in certain Plains and Great Basin tribes, occur myriad intermediate gradations, intermediate mixtures in various proportions of the private and the communal elements. All land systems are alike and uniform in so far as they are blends of both private and communal elements.

We now come to our third uniformity, that of limitations. Even where the integral political entity turns over part of its land or the use thereof to individuals or fractional groups, it does so under limitations and restrictions. It makes reservations. These limitations, restrictions, and reservations concern firmness and duration of tenure, and freedom to exploit, enjoy, and alienate.

Among non-literate peoples, these limitations and restrictions are many, differing, as is well known, from people to people, but are a phenomenon common to all such peoples. Some of the more common limitations are: "ownership" amounting to little more than "usufruct"; tenure only during occupation or use; absence of right or, at least, custom of sale; use, exploitation, and enjoyment restricted to a very few specific land-linked resources and facilities.

Our own modern American tenure in fee simple, one of the least restricted forms of land tenure, perhaps the least restricted, in the career of human culture, is nevertheless cabined, cribbed, and confined in scores of ways. To list only a few of these limitations, restrictions, and reservations: the power of eminent domain; the police power, which may enforce, *e g.*, laws of zoning and nuisance abatement; the taxing power, which may demand high inheritance taxes on landed estates or perhaps even capital levies thereon; rights of transit across owned land, and similar easements; public rights of use of navigable streams running through privately owned land; air rights across owned land; certain limitations on subsoil rights; limitations on freedom of bequeathment; spendthrift trusts; and so forth. Further, estates in fee tail are still recognized in six of our states. Also, apart from enacted law, what may fairly be called customary law, enforced, not by courts, but by public opinion, brings strong pressure to bear upon certain classes of land holders against misuse of their land or against alienation thereof to other than the holders' offspring or near kin; and, since a year ago unenforceable at law, yet still widely prevalent in many large sections of our country, are restrictive convenants held in force by extralegal local sentiment and approval.

Limitations, restrictions, and reservations, as regards both private ownership and private rights of alienation and use, are consistently recurring phenomena, characteristic, it seems, of all systems of land tenure known to us. Where such limitations go far, whether in non-literate political entities or, for example, in pre-war Nazi Germany, such private tenure as obtains may be called indifferently either "ownership" or "usufruct." In few systems can a sharp line be drawn. We as anthropologists would probably do well, in the interests of clarity, to scrap both terms and keep to the neutral one, "tenure."

A fourth uniformity, the last to be dealt with, is the notion, implicit or explicit, of trusteeship that appears, we believe, in all land tenure systems.

Explicit recognition of such trusteeship is a commonplace in non-literate societies, as, *e.g.*, in the numerous cases where the integral political entity's land is vested in the chief or tribal council to be held in trust for the people and to be administered for and distributed to them as need arises, or, where land owned by an individual or by a fractional group must, by customary law, be held in trust for the individual's descendants or heirs, or for present and future members of the group.

The notion of trusteeship is likewise present, in part implicitly and in part explicitly, in our own Occidental system of land tenure, including our modern American one. It is at least implicitly present in the power of eminent domain, in the police power, and in many of the limitations, such as some estates in fee tail, which we have listed. It is explicit in the Christian tradition of property, including landed property, as a stewardship or trusteeship. Under this tradition, an historic component of our Occidental culture and operating mostly on a non-legalistic level, surplus goods and property, that is, such as are not needed to meet the legitimate wants of the owner, must be shared with "the poor," those who need, but lack such resources and facilities required for a decent standard of living.

To sum up the present brief paper, we have attempted to analyze and formulate four of the basic uniformities in the concepts of land tenure which prevail in the cultures of the world. These four uniformities are:

(1) A triple dynamic—psychological, social, and ecological—lies back of all systems of land tenure.

(2) Each system works under a double selective process: (a) selecting some land-linked resources and facilities for exploitation and enjoyment, and neglecting others; and (b) of those resources and facilities chosen, selecting some for free use by all members of the political entity, and permitting exclusive tenure and use of others to individuals and fractional groups.

(3) Where exclusive tenure and use are granted to individuals and fractional groups, limitations and restrictions of many kinds are imposed upon such tenure and use, the

political entity reserving much for the benefit of other individuals or fractional groups.

(4) An explicit or implicit notion of trusteeship, on the part of the integral political entity and/or on the part of individuals and fractional groups, is extremely widespread in land tenure systems and is probably present in all of them.

SECTION OF PHYSICS AND CHEMISTRY

PRECISION COLORIMETRY

By C. F. HISKEY*

There have been two general ways by which the precision of colorimetric analyses has been improved. One of these utilized the design of sensitive photoelectric circuits capable of detecting as little as 0.00001 of the optical scales. This has been done by such workers as Von Halban, Kortüm, Rabinowitch, and Klotz and Dole. The other approach involved a detailed investigation of the errors of transmission measurements, so that these being understood, they then could be minimized. This method is associated with the names of Von Halban, Twyman and Lothian, Kortüm, Ringbom, and Hamilton. From these workers, our present approach to the measurement of the relative transmittance of a solution has been derived.

In general, however, their colorimetric procedures involved a comparison of a solvent with a solution. The measurement of relative concentrations, which requires a measurement of a transmission ratio, has not been extensively investigated. Such is the purpose of this study.

The Transmission Ratio. Consider a situation where three beams of light, of identical frequency and intensity, are permitted to fall on three identical absorption cells which contain, respectively, pure solvent, colored solution of concentration C_1, and colored solution of concentration C_2 (Figure 1).

A comparison of the various exit light intensities of the solutions with that of the pure solvent, would give the following relations;

$$I_1 = I_0 e^{-kC_1 l};\ I_2 = I_0 e^{-kC_2 l}. \qquad (1)$$

These are, of course, the Beer-Lambert expressions for those situations where comparison is made against the solvent being used.

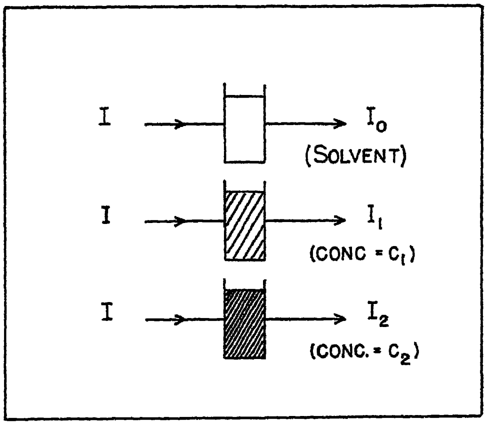

Figure 1

The ratio of the light intensities leaving the solutions of concentrations C_2 and C_1 would be:

$$\frac{I_2}{I_1} = \frac{I_0 e^{-kC_2 l}}{I_0 e^{-kC_1 l}} = e^{-kl(C_2-C_1)} = e^{-kl(\Delta C)}. \qquad (2)$$

This will be termed the transmission ratio. It is apparent that it is the concentration difference between the two solutions which determines the value of I_2/I_1.

If the concentration difference now is expressed in terms of the concentration ratio α, *i.e.*, C_2/C_1, the following expressions apply:

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$$\frac{C_2}{C_1} = \frac{C_1 + \Delta C}{C_1} = \alpha, \qquad (3)$$

also

$$\Delta C = C_1(\alpha - 1) = C_2\left(\frac{\alpha - 1}{\alpha}\right)$$

$$I_2/I_1 = e^{-klC_1(\alpha-1)} = e^{-E_1(\alpha-1)}. \qquad (4)$$

EQUATION 4 indicates the relationship which would prevail if the transmission of the solution under analysis was measured by comparison against a solution of the same substance whose concentration was known. This is different from ordinary colorimetric practice. It requires that a standard solution be placed in one cell of a null type colorimeter, with the light intensity adjusted to give full scale deflection of the galvanometer, and in the other cell, the unknown solution is placed, followed by balancing of the system by what ever circuit principle is used. Providing a light source of sufficient stability is part of the colorimeter, a single photo cell instrument may also be used with substitution of the unknown solution after the instrument is set for maximum deflection by means of the color standard.

From an examination of EQUATION 4 the important role of the extinction E_1 of the standard solution becomes apparent. This is demonstrated in the plot of I_2/I_1 *vs.* E_1 given in FIGURE 2. The plots are made for different values of the concentration ratio α.

It is evident that with increasing extinction in the reference standard the transmission ratio becomes increasingly sensitive to the ratio of the concentrations being compared. Even when α is only 1.01, it is still possible to demonstrate that fact without much difficulty by working with a reference standard whose extinction is somewhat more than unity. For a standard whose extinction is 4.0, I_2/I_1 would be equal to 0.96, and if the colorimeter scale could be read to 0.005, then a ratio as low as 1.0012 ought to be detectable.

It will next be useful to compare the precision of measurement in ordinary colorimetry with that of the transmission ratio function. In this way, it will be possible to ascertain the conditions requisite for increased precision.

Precision of Transmission Ratio Measurements. The error functions of interest are those involving the ratio of C_2/C. It may be shown that:

$$\frac{d(\alpha - 1)}{(\alpha - 1)} = \frac{d(\Delta C)}{C}; \qquad (5)$$

$$\frac{d\alpha}{\alpha} = \frac{d(\alpha - 1)}{\alpha} = \frac{dC_2}{C_2}. \qquad (6)$$

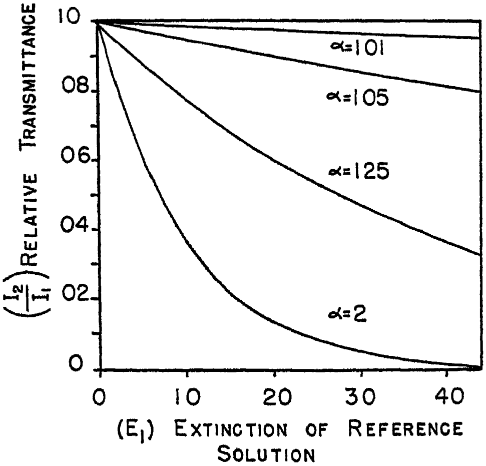

Figure 2

By deriving the error functions in terms of I_2/I_1 expressions (7) and (8) may be obtained.

$$\frac{d(\Delta C)}{\Delta C} = \frac{d(I_2/I_1)}{I_2/I_1 \ln I_2/I_1}. \qquad (7)$$

$$\frac{dC_2}{C_2} = \frac{d(I_2/I_1)}{\frac{I_2}{I_1}\left(\ln\frac{I_2}{I_1} + \ln\frac{I_1}{I_0}\right)}. \qquad (8)$$

The value of these error functions may be approximated by substituting small finite increments for the infinitesimals. $\Delta\ (I_2/I_1)$ is the uncertainty of reading the exact value of the transmission ratio on the colorimeter scale. It arises from the fact that the setting at some value of the transmission ratio is always uncertain by a constant quantity, which in most commercial instruments will be of the order of 0.01 to 0.002 of the total scale. $\Delta\ (I_2/I_1)$ is, therefore, a constant quantity for any particular colorimeter being used. It may be evaluated experimentally by taking a standard and an unknown, and measuring the transmission ratio repeatedly. Each measurement should include the operations of emptying, refilling, and repositioning the cuvettes. $\Delta(I_2/I_1)$ may be taken as twice the value of the average deviation.

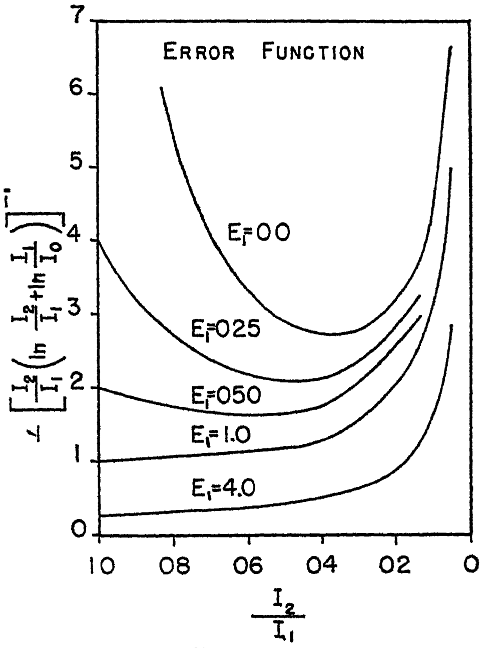

Figure 3

In FIGURE 3, a plot of the reciprocal of $I_2/I_1 \ln I_2/I_1$ is given as a function of the transmission ratio. The curve marked $E_1 = 0.0$ is the only one under consideration. The value of $\Delta\ I_2/I_1$ is left unspecified. Here it can be seen that the relative error is infinite for 0.0 and 100 per cent transmission ratios. At intermediate values, the relative error decreases to a minimum and then rises again. In the region between 20 and 65 per cent, the curve is nearly flat and, hence, a minimum and nearly constant error is achieved by working in this region.

Differentiating the expression with respect to I_2/I_1 and setting the function equal to zero gives the point of minimum error which is found to correspond to a value of 0.368. The numerical values of $d(\Delta\ C)/\Delta C$ can be shown to be equal to those of dc/C when the concentration is determined by ordinary colorimetric methods. In other words, the determination of a concentration difference is no more precise than is the determination of a concentration by comparison against the solvent.

In chemical analysis, however, the prime interest is in the value of the total concentration and, therefore, the main concern is with the error involved in that measurement. This error is seen (from an inspection of EQUATION 8 and the graphical plots in FIGURE 3) to be dependent in a pronounced manner on the value of the extinction of the comparison standard.

In these graphs, also, only the reciprocal of $I_2/I_1(\ln I_2/I_1 + \ln I_1/I_0)$ is plotted on the ordinate instead of dC_2/C_2. In this way, the graph is generalized to cover all photoelectric colorimeters, since the value of the term $\Delta\ (I_2/I_1)$ is not specified. Curves are given for a number of values of the extinction of the standard solution, *i.e.*, $E_1 = 0.25, 0.50, 1.0$ and 4.0.

Now it is seen that as soon as the value of E_1 exceeds zero the value of the relative error becomes finite at the 100 per cent end of the colorimeter scale. At all other values of the scale, the error for any given extinction of the standard is less than is the case with solvent comparison. As the value of the transmission ratio approaches zero, the entire group of curves approach infinity. Therefore, little gain in precision is to be expected in that region of the scale. Fortunately, that region need not be used in any analytical operation. However, in the 100 per cent region, the decrease in error is extremely rapid with increasing extinction of the standard. In FIGURE 4, a plot (solid line) is given for the error involved in measuring the concentration of an unknown when its concentration is identical with that of the standard. This plot is made as a function of the per cent transmission of the standard.

Here, it can be seen how rapidly the error declines as the extinction increases. When the standard has a transmission of as much as 0.69, the relative error of making this particular measurement is identical with minimum error achieved in ordinary colorimetry, and, for the other values of I_2/I_1 at the same transmission for the standard, the error is even less. It remains lower until I_2/I_1 decreases to about 0.20, at which point it again becomes equal to the minimum error of ordinary colorimetric practice. Nevertheless, there is always a relative gain in precision by working against a standard solution rather than a solvent.

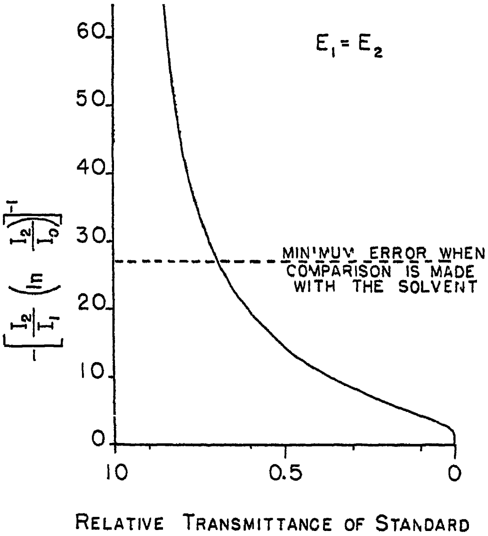

Figure 4

It is also evident from FIGURE 3 that the position of minimum error progressively moves from 0.368 on the transmission ratio scale to 1.0 as the extinction increases. The exact position of this minimum error may be obtained by differentiating the function with respect to I_2/I_1 and setting it equal to zero. When this is done,

$$\ln\frac{I_2}{I_1} + \ln\frac{I_1}{I_0} + 1 = 0;$$

also

$$\ln\frac{I_2}{I_1} + 1 = 0$$

and

$$\frac{I_2}{I_1} = e^{-(1-E_1)}. \tag{10}$$

Here, it is seen that as the extinction of the standard increases the value of I_2/I_1 steadily increases, and when the extinction equals one, the minimum error is obtained when I_2/I_1 equals one, *i.e.*, at identical extinctions.

Were it not for the rapid decrease in light transmission with increasing extinction, the error could be reduced at will. In practice, it will probably be impractical to work with an extinction much in excess of four, because the light transmission would be only 1.8 per cent.

With this limitation recognized, however, a considerable gain is still achieved in the precision. Examination of the plot shows that the minimum error when $E_1 = 0$ is equal to 2.72. This occurs when the extinction of the unknown is 1.0 or has a transmission of 36.8 per cent. In the case where E_1 has a value of four, the minimum error is 0.25, or about one eleventh that obtained with the solvent as the comparison standard. In this case, however, the extinctions of the unknown and the standard must be made nearly equal to achieve the precision gain.

This general treatment of error applies only to those cases where the Beer-Lambert relation holds and where the chemical operations have greater precision than those of the colorimetric measurements. In practice, of course, solutions of high extinction may often be of relatively high concentration. In such cases, deviations from the Beer-Lambert expression may become apparent.*

Application of the Transmission Ratio Method. The utility of the transmission ratio method of colorimetry may be demonstrated from the published work of Kortüm and Bastian. In a series of measurements of the extinction coefficient of 2,4 dinitrophenol solutions, Kortüm observed that greater precision could be obtained in determining concentrations by making a comparison of an unknown with a standard than could be obtained in an absolute measurement of the extinction. His data in part are listed in Table 1.

TABLE 1

COMPARISON OF THE TWO METHODS OF COLORIMETRY ON 2,4 DINITROPHENOL.

	I_1/I_0	I_2/I_0	$E_2 = \log_{10} I_0/I_2$	C_2 *Dtn. by transmission Ratio Method*
First day				
	.37072	.36214	.44112	4.982×10^{-5}
	.37100	.36254	.44064	4.980
	.37114	.36280	.44033	4.979
Average	.37095 ±.00016	.36249 ±.00024	.44139 ±.00028	
Second day				
	.36940	.36108	.44239	4.979
	.36968	.36130	.44213	4.980
	.36994	.36164	.44172	4.979
Average	.36967 ±.00018	.36134 ±.00020	.44208 ±.00024	4.980 ±.001 } Av. of all 6

If the average deviations from each of the four sets of transmission data are averaged, it will be found that $\Delta\ I/I_0 = \pm.00020$. The values of $\frac{\Delta E_2}{E_2}$, on the ordinary colorimetric basis, and the $\frac{\Delta\ C}{C_2}$, by the transmission ratio method, now may be calculated and found to be ±.00055 and ±.00020, respectively. The observed values averaged for the two sets of extinction and concentration values are ±.00059 and ±.00020, respectively. These are seen to be in good agreement with the experimental evidence.

It will be observed, however, that between the transmission data of the two days there is an absolute shift of about 0.0012, due perhaps to some alteration in the optical or electrical circuit. This creates a large spread in the values for E_2, but is virtually without effect on the value of C_2 determined by the transmission ratio. Thus, in another way, the advantages of this method are demonstrated.

More recently, Bastian applied the transmission ratio method to the determination of copper in various copper base alloys. The instrument used by him was the Beckman spectrophotometer (Model Du). In this instrument, the optical scale may be extended by opening the slits so that a larger section of the spectrum is passed through the absorption cell. Working with solutions whose extinctions were about 2.5, he was able to make analyses of copper in lead brasses, phosphor bronzes, and synthetically prepared unknowns with a precision of ±.001 and ±.002. There is evidence in this work that the limits of precision are determined, not by the colorimetric measurements, but, rather, by the chemical operations. Nevertheless, the precision of his work, as well as the accuracy, (assuming the electrolytic values to be correct, the agreement obtained colorimetrically was always within one to two parts per thousand), far surpasses any other colorimetric process previously reported.

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* A treatment of the error function when the system deviates from the Beer-Lambert equation has been made. It will appear shortly, along with a consideration of refractive index and density changes in the solutions.

In a future publication, data on the performance of a number of commercial photoelectric colorimeters will be published. In a preliminary way, it may be said that tests on solutions of a number of salts, *i.e.*, potassium dichromate, copper sulfate, and on various dyes confirm the utility of this approach.

Method as Applied in Commercial Colorimeters. To apply the above principle to commercial photo electric colorimeters or spectrophotometers, it is a requisite condition that the instrument used must have a variable optical scale. This variation is achieved in practice in a number of different ways. One method is that of adjusting the slits on a monochromator so that a larger fraction of the spectrum is passed through the solution. A second, is to vary the current passing through the light source and thus increase the emission intensity. A third method is to operate the indicating device, *i.e.*, galvanometer, *etc.*, with a variable sensitivity. Thus, if only a fraction of the total current is fed to a galvanometer during measurements on solutions of low extinction, then when the shift is made to higher extinctions, the shunt may be adjusted to pass an increasing fraction of the photo current through the galvanometer and, thus, effectively increase the optical scale.

The transmission ratio measurement is then made with a pair of optically matched cuvettes each containing one of the solutions to be compared. Placing the standard solution in the monochromatic (or at least filtered) light beam, adjustment of the optical scale is made until the instrument reads 100 per cent transmission. The analyst must then make sure, at this point, that the lower end of the scale is at zero. With the instrument now properly set, the second solution is placed in the light beam, whereupon, if its extinction is greater than that of the standard solution, the new value read will be some value less than 100. It will equal 100(I_2/I_1). Should the extinction of the second solution be less than that of the standard, the reading of the I_2/I_1 value will be off the scale, since it will be in excess of 100. To make the comparison then, it is necessary to reverse the order of the two cells.

There are several precautions to bear in mind in this type of colorimetry. In the first place, the optical characteristics of a pair of cuvettes must be known with a precision equal to the best precision anticipated for the work. This will involve comparing the cuvettes when they contain a solution of high, but identical, extinction in them. The extinction of the solution will obviously be the maximum for which full scale deflection can be achieved. Then, the following terms may be applied:

$$\frac{I_2}{I_1} = e^{-kcl_1(L-1)} \quad \text{where} \quad L = \frac{l_2}{l_1},$$

i.e., the ratio of the apparent path lengths in the two cells. This ratio, once evaluated, is then applied as a correction term to all other work. It, too, should be checked from time to time, since alterations of the surfaces of the optical flats will change L.

Next, it will be necessary to determine the extinction of the standard solution and to prepare a precise calibration curve for the purpose of seeing the agreement with the Beer-Lambert equation. This should be done in the following manner. Prepare a number of standard solutions covering the range of extinctions desired. If, for example, it is one's intention to work from 0.1 to 3.0, then a thirty-fold concentration range would be needed. Start with pure solvent in one cell and successively measure the transmissions (I_1/I_0) of the solutions until the value of I_1/I_0 has dropped to about 0.37. Then remove the solvent from the cuvette and substitute in it some of the solution which had the 0.37 value. Compare the two cuvettes to see if I_2/I_1 equals unity. If not, a preliminary value of L will thus be obtained.

Continue measuring I_2/I_1 values for the more concentrated solutions. Proceed in this way until the value of $I_2/I_1 \cong 0.37$. This means that the transmission of the solution being examined is only 0.135 with respect to the solvent, although it is 0.37

with respect to a solution whose own transmission is only 0.37. Again, L is evaluated by changing our standard so that it is identical with the one already in the other cuvette. Taking this as the second reference point, I_2/I_1 values may be measured for the more concentrated solutions. In this way, if the Beer-Lambert equation applies, the precision of measuring the slope relating E to C will be kept at the maximum value possible for the instrument and the extinction chosen.

When measurements for the entire concentration range which the instrument can handle have been made, the value of L will be finally evaluated, and with maximum precision, using the solution of highest concentration.

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CONFERENCES HELD

SECTION OF BIOLOGY

CONFERENCE ON "COCCIDIOSIS"

MARCH 4 AND 5, 1949

The Section of Biology held a Conference on "Coccidiosis." Doctor Sterling Brackett, American Cyanamid Company, Stamford, Connecticut, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, March 4

Morning Session. Chairman, Norman R. Stoll.

"Introductory Remarks by the Conference Chairman," Sterling Brackett.

"Comments by the Honorary Chairman", E. E. Tyzzer, Harvard University, Cambridge, Mass.

"The Economic Losses Due to Coccidiosis," by A. O. Foster, Zoological Division, U.S.D.A., Beltsville, Maryland.

"Special Problems in Coccidiosis of Chickens in Different Regions of the United States,"—P. P. Levine, Moderator. A Round Table Discussion, by R. A. Bankowski, C. W. Barber, J. P. Delaplane, L. C. Grumbles, P. A. Hawkins, E. H. Peterson and K. C. Seeger.

"Some Factors Influencing the Origin and Course of Epidemics of Coccidiosis," by C. Horton-Smith, Animal Health Trust, Poultry Research Station, Houghton Grange, England.

"The Course of Coccidiosis Epidemics in Broilers in Delaware," by K. C. Seeger, University of Delaware, Field Station, Georgetown, Delaware.

"Effect of Litter Moisture on Outbreaks of Coccidiosis," by A. E. Tomhave, University of Delaware, Newark, Delaware.

"Laying House Coccidiosis Due Presumably to *Eimeria acervulina*," by E. H. Peterson, State College of Washington, Pullman, Wash.

Afternoon Session. Chairman, Elery R. Becker.

"Survival of *Eimeria acervulina*, *E. maxima* and *E. tenella* Oocysts Exposed on Soil under Various Field Conditions," by M. M. Farr and E. Wehr, Zoological

Division, U.S.D.A., Beltsville, Maryland.

"Some Host Parasite Relationships between *Eimeria tenella* and Chickens," by C. A. Herrick, C. A. Ripsom, C. A. Johnson, and J. W. Daugherty, University of Wisconsin, Madison, Wisconsin.

"Factors Involved in Tests for Acquired Immunity in *Eimeria tenella* Infections of the Chicken," by E. Waletzky and C. O. Hughes, American Cyanamid Company, Stamford, Conn.

"The Effect of the Protozoan Parasite, *Eimeria stiedae*, on the Succinic Dehydrogenase Activity of Liver Tissue of the Rabbit," by B. F. Smith, Spelman College, Atlanta, Georgia; and W. H. McShan, Department of Zoology, University of Wisconsin, Madison, Wisconsin.

"Coccidiosis as a Disease in Turkeys," by N. F. Morehouse, Doctor Salsbury's Laboratories, Charles City, Iowa.

"The Development of Naturally Acquired Coccidial Infections in Lambs," by J. S. Dunlap and P. A. Hawkins, Department of Bacteriology, Michigan State College, East Lansing, Michigan.

"Coccidiosis in Calves,"—A Moving Picture, by L. R. Davis, Regional Laboratory, U.S.D.A., Auburn, Alabama.

Saturday, March 5

Morning Session. Chairman, F. Y. Wiselogle

"The Relative Efficacy and Safety of New Drugs for Poultry Evaluated by Adequately Controlled Tests," by J. H. Collins, Veterinary Medical Section, F. D. A., Federal Security Agency, Washington, D. C.

"Qualitative and Quantitative Comparisons of Different Types of Anticoccidial Drugs in *Eimeria tenella* Infections of the Chicken," by E. Waletzky and C. O. Hughes, American Cyanamid Company, Stamford, Connecticut.

"Suppressive Activity of Bisphenols Against Cecal Coccidiosis (*Eimeria tenella*) in Chickens," by J. E. Johnson, R. D. Mussell, and A. J. Dietzler, Biochemical Research Laboratories, Dow Chemical Company, Midland, Michigan.

"Para-substituted Phenylarsonic Acids as Prophylactic Agents Against *Eimeria tenella* Infections," by F. C. Goble, Sterling-Winthrop Research Institute, Rensselaer, N. Y.

"Nitrofurazone and Coccidiosis," by P. D. Harwood and D. I. Stunz, Dr. Hess and Clark, Ashland, Ohio.

"The Anticoccidial Activity of Nitrophenide," by E. Waletzky and C. O. Hughes, American Cyanamid Company, Stamford, Connecticut.

"Additional Information on the Use of Sulfaquinoxaline for the Control of Coccidiosis in Poultry," by L. C. Grumbles, Louisiana State University, Baton Rouge, Louisiana.

"Continuous Low Level Sulfaquinoxaline Feeding in the Practical Control of Coccidiosis in Broilers," by E. L. Jungherr and J. D. Winn, University of Connecticut, Storrs, Connecticut.

Afternoon Session. Chairman, P. P. Levine.

"Borax in the Control of Experimental *Eimeria tenella* Infection of Chickens," by E. E. Wehr, Zoological Division, Bureau of Animal Industry, Beltsville Research Center, Beltsville, Maryland.

"The Effect of Various Agents in the Treatment of Experimentally Induced Coccidiosis (*Eimeria tenella*) in Chickens," by K. C. Seeger, University of Delaware, Field Station, Georgetown, Delaware.

"The Chemotherapy of Coccidiosis Due to *Eimeria acervulina*," by E. H. Peterson, State College of Washington, Pullman, Washington.

"Growth and Mortality of Chickens as Affected by the Floor Litter," by D. C. Kennard and V. D. Chamberlin, Poultry Science Department, Ohio Agricultural Experiment Station, Wooster, Ohio.

"Preliminary Experiments with Sulfamethazine in Bovine Coccidiosis," by L. R. Davis and G. W. Bowman, Regional Laboratory, U.S.D.A., Auburn, Alabama.

"The Effect of Small Doses of Drugs on

the Oocyst Production of Infections with *Eimeria tenella*," by Sterling Brackett and A. Bliznick, American Cyanamid Company, Stamford, Connecticut.

"Consideration in the Practical Control of Intestinal Coccidiosis of Domestic Rabbits," by E. E. Lund, U. S. Rabbit Experiment Station, Fontana, California.

"Coccidiosis in Native California Valley Quail and Problems of Control," by C. M. Herman, California Division of Fish and Game, University of California, Berkeley, California.

SECTION OF PHYSICS AND CHEMISTRY

CONFERENCE ON "SPECIFIC METHODS OF ANALYSIS" MARCH 25 AND 26, 1949

The Section of Physics and Chemistry held a Conference on "Specific Methods of Analysis." Doctor Samuel E. Q. Ashley, General Electric Company, Pittsfield, Massachusetts, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, March 25

Morning Session. Chairman, Samuel E. Q. Ashley.

"Introductory Remarks by the Conference Chairman," Samuel E. Q. Ashley.

"X-ray Absorption and Chemical Analysis," by Hermann A. Liebhafsky, Research Laboratory, General Electric Company, Schenectady, N. Y.

"The Analytical Specificity of Counter Current Distribution," by J. Delafield Gregory and Lyman C. Craig, Rockefeller Institute for Medical Research, New York, N. Y.

"The Method and Application of Differential Thermal Analysis," by Ralph E. Grim, State Geological Survey Division, State of Illinois, Urbana, Illinois.

Afternoon Session. Chairman, Leland A. Wooten.

"Optical Properties of Surface Films," by Alexandre Rothen, Rockefeller Institute for Medical Research, New York, N. Y.

"Electrolytic Analysis of Tarnish Film on Metals," by Wilfred E. Campbell, Bell Telephone Laboratories, New York, N. Y.

"Measurement of Properties of Thin Films on Chromium by the Reflection of Polarized Light," by J. B. Bateman and Margaret W. Harris, Camp Detrick, Maryland.

"Goniometric Analysis of Crystals," by J. D. H. Donnay, The Johns Hopkins University, Baltimore, Maryland.

Saturday, March 26

Morning Session. Chairman, N. Howell Furman.

"Quantitative Analysis by Means of Catalytic Hydrogenation Reactions," by E. D. Dunlop, Experimental Station, E. I. duPont de Nemours and Company, Inc., Wilmington, Delaware.

"Analysis of Mixtures Based on Rates of Reaction," by Thomas S. Lee, Department of Chemistry, University of Minnesota, Minneapolis, Minnesota.

"Automatic Paper Chromatography" by, Ralph H. Müller and Doris L. Clegg, Department of Chemistry, Washington Square College of Arts and Sciences, New York University, New York, N. Y.

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Elected March 24, 1949

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TRANSACTIONS OF THE NEW YORK ACADEMY OF SCIENCES

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ROY WALDO MINER

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Executive Director: EUNICE THOMAS MINER

SECTION OF PHYSICS AND CHEMISTRY[*]

AN APPROXIMATE SOLUTION OF CERTAIN DIFFUSION PROBLEMS

By HOWARD REISS[†]

Introduction. This paper contains a description of two problems representative of those connected with the quantitative investigation of the procedures used to prepare monodispersed aerosols and hydrosols. The manner of solution of these problems will be indicated and the results will be discussed. The author hopes to create some interest in a field which just recently became quantitative.

A longer, more mathematical paper is planned for publication in the near future.

Aerosols and Spontaneous Nucleation. Monodispersed aerosols can be produced by the method of La Mer and Sinclair.[1] A uniform mixture of air, vapor, and condensation nuclei (small grains of foreign material) is cooled by passage through a column along which a temperature gradient is maintained. Upon the nuclear surfaces, the mixture cannot become supersaturated with vapor, and condensation results. Vapor from the internuclear regions diffuses to the surfaces and each nucleus is surrounded by a growing liquid droplet. The product is a monodispersed aerosol.

In order to maintain a high degree of monodispersity it is desirable for all the droplets to begin to form simultaneously. The occurrence of *spontaneous* nucleation which, at a later time, supplies nuclei additional to the foreign nuclei originally present is, therefore, to be avoided.

These nuclei, formed spontaneously, are merely fragments of the condensed phase which appear in large numbers when the degree of supersaturation exceeds a certain critical limit. Becker and Boring[2] have discussed the phenomenon quantitatively in terms of a *supersaturation* ratio: the ratio, at a given temperature, of the vapor pressure in the supersaturated state to that in the saturated state. The critical ratios, signalizing the onset of spontaneous nucleation, calculated by these authors, are in good agreement with experiment.

If the air-vapor-nuclei mixture, previously mentioned, is cooled rapidly, it is conceivable that the diffusion process will not be able to drain vapor from the internuclear regions fast enough to prevent the supersaturation ratios for these regions from exceeding the critical limit. Whether or not this will happen can only be decided when the diffusional boundary value problem has been solved so that the concentration of vapor in these regions can be computed at every instant.

The Growth Rate of Sulfur Hydrosols. Monodispersed sulfur hydrosols can be produced by the method of La Mer and Barnes.[3] Dilute solutions of acid and thiosulfate are mixed, whereupon the following reaction occurs

$$2HS_2O_3^- = 2S + 2HSO_3^-.$$

At first, the sulfur is released in solution. As the reaction proceeds, the solution becomes supersaturated with respect to sulfur and a critical limit is reached. At this point, spontaneous nucleation occurs, and the sulfur in solution diffuses towards the newly formed sinks. If the diffusion process is sufficiently rapid, it will remove dissolved sulfur more rapidly than the chemical reaction can produce it, and the concentration will be lowered below the critical limit, bringing an end to the nucleation process. During the final stage, the sulfur droplets grow by means of diffusion in the field of a uniformly distributed source, *i.e.* the chemical reaction.

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[*] The Section of Geology and Mineralogy held its meeting on April 4, 1949, at which Ellis A. Johnson presented a paper, illustrated with lantern slides, entitled *The Magnetisation of the Earth's Crust.* This will be published in a later issue of TRANSACTIONS.

[†] Department of Chemistry, Columbia University, New York, N. Y. This paper was presented at the meeting of the Section on April 5, 1949.

It is of interest to know whether or not the reaction is catalyzed at the droplet surfaces, *i.e.* can the growth of the droplets be ascribed partly to deposition from an accelerated reaction at their surfaces, or is it simply describable in terms of the diffusion of molecularly dispersed sulfur?

Here, again, a decision can only be arrived at by the computation of the droplet growth rates upon the basis of a diffusion mechanism and by comparing the result with experimental data.

Methods of Solution. The aerosol and hydrosol problems, described previously, contain basic elements of similarity. Both involve diffusion towards many randomly located sinks (nuclei), and both are peculiar in that a boundary, namely the droplet surface, is not geometrically fixed but moves as the droplet grows. Because of these facts, the problems cast into their natural forms are too difficult to solve. It is, therefore, expedient to replace them by artificial problems which retain the important features of the natural ones, but which are amenable to solution.

Instead of considering a large number of nuclei, it is practicable to confine attention, in each case, to the neighborhood of a single nucleus, and regard this neighborhood to be spherically symmetric. The presence of the other nuclei, which compete for the diffusing material, can be accounted for by enclosing the neighborhood in an impermeable spherical shell of radius h, centered at the nucleus whose radius is x. The impermeability of the shell prevents the material beyond its confines from depositing on the central nucleus, so that, in effect, this material may be considered to condense on the competing nuclei. The magnitude of the radius h is chosen so that the volume contained in the shell equals the reciprocal of the number density of the nuclei, *i.e.* the effective volume of the diffusion field belonging to each nucleus.

The spherical symmetry renders it sufficient to determine position by the magnitude, r, of the radius vector originating at the center of the nucleus of radius, x.

In the aerosol case, the initial concentration of vapor c_0, corresponding to the temperature, T_0, is everywhere uniform. The boundary condition at x corresponds to the maintenance, there, of the saturation pressure.

In the hydrosol case, c_0 represents the concentration of sulfur molecules when the critical limit of supersaturation is reached. The boundary condition at x corresponds to the maintenance of saturation, *i.e.* the concentration at x is always equal to the solubility of sulfur. In the hydrosol case, the diffusion equation includes a source term, corresponding to the ever-present chemical reaction.

In both cases the movement of the boundary at x makes it impossible to use standard methods to solve the diffusional boundary value problems. Ordinarily, the equation governing the diffusion phenomena is

$$\frac{\partial^2 c}{\partial r^2}+\frac{2}{r}\frac{\partial c}{\partial r}=\frac{1}{D}\frac{\partial c}{\partial t}-\frac{k}{D}, \tag{1}$$

where c is the concentration of the diffusing species. This equation is valid, provided that Fick's law[4] holds. D is the diffusion coefficient, t, the time, and k is the source function equal to zero in the aerosol case. In certain physical situations, the flux of material through any surface in the diffusion field is much greater than the rate of change of concentration on that surface. In such cases, (1) can be replaced by the approximation

$$\frac{\partial^2 c}{\partial r^2}+\frac{2}{r}\frac{\partial c}{\partial r}=-\frac{k}{D}. \tag{2}$$

EQUATION (2) is equally simple to solve, independently of whether the boundary does or does not move. It happens that (2) is admissible whenever x/h is small. The simplifying feature in the problem involving (2) is the fact that the time dependence of c is not determined by (2), but, rather, by the boundary conditions.

The condition for an impermeable shell at h is ordinarily

$$\left(\frac{\partial c}{\partial r}\right)_{r=h} = 0 \tag{3}$$

The only solution of (2), compatible with (3) is the trivial solution

$$c = c(t) \tag{4}$$

which implies that c does not depend upon r but only upon t. It is, therefore, necessary to replace (3) by a more reasonable condition.

This can be done by realizing that the impermeable shell limits the amount of material available for diffusion. Consequently, the amount of material diffusing into the surface at x represents, exactly, the amount of material lost from the region between the boundaries at x and h. The maintenance of this balance can be worked into a condition suitable for the replacement of (3).

The rate at which x changes is, of course, determined by the process of diffusion. It is given by

$$\frac{dx}{dt} = \frac{WD}{A\rho}\left(\frac{\partial c}{\partial r}\right)_{r=x} \tag{5}$$

where W is the molecular weight of the diffusing species, A is Avogadro's number, and ρ is the density of the droplet.

Results. The solution of the aerosol problem yields the value of c (r, t). This value will always be highest at $r = h$, since diffusion occurs towards the sink at x. Consequently, if spontaneous nucleation is to take place, it will take place at h, first. It was found convenient to define a *cooling* ratio

$$c(h, t)/c(x, t). \tag{6}$$

Since c (x, t) is the saturation concentration, spontaneous nucleation would be predicted whenever (6) exceeded the critical ratio computed by the method of Becker and Doring. It was useful to plot both the critical ratio and cooling ratio against time (temperature) on the same graph. The intersection of the two curves predicts spontaneous nucleation.

Calculations and plots of these types were made for naphthalene aerosols which happen to be similar to a number of sols prepared in this laboratory. The calculations were carried out for different cooling rates, different concentrations of foreign nuclei, and different values of T_o. The results indicate that so long as there are more than 100 nuclei per cc. whose surfaces cannot support supersaturation, spontaneous nucleation cannot occur during the preparation of a naphthalene-like aerosol, even when the cooling rate is ten times normal, *i.e.* 530° K./sec.

The aerosols produced in this laboratory contain about 10^6 nuclei per cc. The comparative ease with which a monodispersed aerosol can be produced supports the above conclusion.

The solution of the hydrosol problem yields the dependence of x upon time. Zaiser and La Mer,[5] using light scattering methods, have measured this dependence. They plotted four curves (corresponding to different conditions) of x^2 against time.

The theoretical functional relationship is

$$x^2 = f(t, W, \rho, k, h, D, c_0). \tag{7}$$

Both D and c_o were not accurately available. However, c_o has been estimated by spectrophotometric means[6] to lie in the range

$$3\text{–}4 \times 10^{-6} \text{ g. atom sulfur/liter.} \tag{8}$$

Furthermore, if it is assumed that the diffusing unit is S_8, D should lie in the range

$$4\text{–}7 \times 10^{-6} \text{ cm}^2\text{/sec,} \tag{9}$$

since all molecules which compare in size with S_8 have diffusion coefficients lying in this range, in aqueous solution at room temperature.[7] Equation (7) could be nicely fitted to the experimental curves if c_o and D were chosen, respectively, to be

$$c_0 = 4.7 \times 10^{-6} \text{ g.atom sulfur/liter} \tag{10}$$

$$D = 2.0 \times 10^{-6} \text{ cm}^2\text{/sec.} \tag{11}$$

These values compare well with (8) and (9) estimated by independent means.

It is to be concluded that the suggested diffusion mechanism for the growth of sulfur droplets upon which equation (7) is based provides an adequate quantitative description of the observed phenomena.

Summary. An approximate method is described for treating problems of diffusion, complicated by moving boundaries and the presence of many competing sinks.

This method is used to investigate problems of the growth of monodispersed sols.

One conclusion is that in a naphthalene-like aerosol system, growing by the diffusion of vapor towards sinks (nuclei) which cannot support supersaturation, spontaneous nucleation will not occur if the number density of nuclei exceeds 100 per cc.

Another conclusion is that the growth of sulfur hydrosols prepared by the method of La Mer and Barnes is not complicated by a surface catalyzed reaction, but may be explained on the basis of a simple diffusion mechanism.

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SECTION OF BIOLOGY

THE RESPONSE OF THE MOUSE ADRENAL GLAND TO STEROID HORMONES: *Report on Recent Investigation*

By JAMES H. LEATHEM*

An adrenal x-zone is present in most strains of mice and lies between the permanent cortex and the medulla of the adolescent male and female, and the nulliparous female.[1,2] Normally, the x-zone of the male disappears about the fifth week, whereas, in the virgin female, it persists for an estimated 80 to 200 days. Pregnancy, however, incites a rapid disappearance of the adrenal x-zone. The tissue of this zone can be identified easily, as the cells are eosinophilic and in cords which interdigitate with the medulla, but, as and when the zone disappears, the interdigitation is lost and a connective tissue capsule forms between the cortex and medulla.

The physiological mechanisms of pregnancy include a marked increase in secretion of estrogen and progesterone. Only during pregnancy is the mouse subjected to a continuous secretion of progesterone and, therefore, this steroid may be responsible for the adrenal x-zone disappearance. In fact, Selye[3] reported x-zone disappearance when 3 mg. of progesterone were injected daily for 20 days, but other investigators, using smaller dosages, but adequate to support pregnancy in spayed mice, failed to support these results. Since large doses of progesterone may have an androgenic action, this may explain Selye's results, and, in our studies, 0.25 mg. of progesterone on alternate days for a month had no effect on the x-zone.

Adrenal excitation may occur during pregnancy and, consequently, secretions from this gland may be altered in such a way as to influence its histology. Indeed, Selye[3] reported x-zone disappearance with desoxycorticosterone acetate (DCA). In our experience, the adrenals of immature female mice are not altered by 0.25 mg. of DCA daily for 30 days or by 1.0 mg. of the steroid daily for 20 days.

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Although estrogen is secreted during the estrus cycle, the level of secretion during pregnancy may be altered sufficiently to elicit x-zone disappearance. A review of the action of administered estrogen[4] favors the view that steroids with estrogenic activity do not influence the x-zone. In our laboratory, stilbestrol, estradiol, estradiol dipropionate, and estrone at different dosage levels failed to alter the x-zone of the immature female, but did induce a ring of cytolysed cells to appear in the neighboring fasciculata. Administration of 2 μg. of stilbestrol daily for 16 days to 24 day old male mice at the start prevented the normal disappearance of the x-zone, presumably, by blocking maturation of the reproductive system.

Frequently, a combination of hormones is more beneficial than one alone and so the combination of estrogen and progesterone was injected into female mice, but adrenal x-zone histology was not influenced.[5]

Since the x-zone tissue disappears in the male mouse concomitant with sexual maturity, it was soon learned that castration prevented this adrenal change. What's more, testosterone propionate as replacement therapy or when administered to normal or spayed female mice quickly deletes the x-zone.[6] Recently, Jones[7] extended these results to include the hypophysectomized castrated mouse. From the accumulated data, the response of the x-zone tissue seems to be specific for androgens and, to date, we have eliminated this adrenal zone in immature female mice with testosterone propionate, methyl testosterone, vinyl testosterone, and androsterone.* These findings prompted us to study the x-zone response to testosterone propionate in assay fashion and compare its sensitivity with that of seminal vesicles and to evaluate the activity of other steroids on this end organ. Seminal vesicle weight response was determined in 22 day old male mice at 72 hours after a single injection of testosterone propionate. Fifty μg. of the steroid induced a 100 per cent increase in seminal vesicle weight and served as a good indication of sensitivity (TABLE 1). For the adrenal x-zone study, 30–32 day old female mice were used and the subcutaneous and intraperitoneal routes of administration investigated. A single injection of 0.5 mg. of testosterone propionate removed the x-zone by day 7 when injected subcutaneously, but no effect was observed after the intraperitoneal injection. Thereafter, testosterone propionate was administered as a single subcutaneous injection and mice killed 7 days later. The adrenals were fixed in Bouin and stained with hematoxylin and eosin. Dosages of 0.05, 0.1, 0.2, 0.3, 0.4, 0.5, 1.0, and 2.5 mg. were studied with the 0.1 mg. dosage proving to be the critical level under our experimental conditions. At the 0.1 mg. level, the x-zone had disappeared or was clearly influenced in all cases, whereas the 0.05 mg. dose did not cause x-zone disappearance. All of the higher dosages abolished the x-zone tissue (TABLE 2).

TABLE 1

Response of immature mice to a single subcutaneous injection of testosterone propionate acting for 72 hours

Treatment	*No. of mice*	*Body wt.* g.	*Testis* mg.	*Seminal vesicle* mg.
Control	30	11.3	76	6.2
0 025 mg.	15	10.3	61	9.6
0 050 mg.	12	12.5	74	13.9

TABLE 2

Effect of a single subcutaneous injection of testosterone propionate on the female mouse x-zone after 7 days

Dosage mg.	*Condition of x-zone*	*Alkaline phosphatase*
None	present	+
0.05	present	0–+
0.10	absent or small	0–+
0.20	absent	+
0.50	absent	++
1.00	absent	
2.50	absent	++++

Recently, Elftman,[8] using the histochemical technique for alkaline phosphatase, observed this enzyme in the cortex of the adult male, whereas the female gland or those adrenals from immature mice were essentially devoid of the enzyme unless treated with testosterone propionate. This interesting finding suggested another mouse adrenal relationship to androgenicity, and a correlated study of x-zone disappearance and alkaline phosphatase appearance was made. Thirty-day-old female mice were given single subcutaneous injections of varied doses of testosterone propionate (Perandren, Ciba). One adrenal was fixed in Bouin and the other in cold 80 per cent alcohol for the enzyme. The x-zone disappearance correlated with the previous study and required 0.1 mg. of the steroid, whereas 0.5 mg. was needed for a definite enzyme increase. Elftman[8] reported an increase in alkaline phosphatase when the same steroid was injected on two successive days in 0.1 mg. daily amounts and the mice killed on day 3. A repeat of this experiment in female mice confirmed an enzyme increase, but x-zone disappearance was complete in only 3 of 7 mice. The problem of time and repeated dosages is under further study.

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* The androgens were generously supplied by Ciba Pharmaceutical Products, Summit, New Jersey

Korenchevsky and Hall[9] observed an androgen antagonism in hyperthyroidism and these data suggested that similar results may be obtained using the x-zone as the end point. Accordingly, hyperthyroidism was induced in 22-day-old mice by putting them on a diet containing 0.1 per cent thyroglobulin (Proloid) for 17 days. The testosterone was administered as a single injection when the mice were 32 days old. In the hyperthyroid state, a 0.2 mg. dose of androgen did not cause x-zone disappearance, but 0.5 mg. was effective showing that the effective dose level of androgen was clearly above the dosage needed in normal mice.

One of the perplexing aspects of the mouse adrenal problem is the disappearance of the x-zone in female mice following treatment with gonadotrophins.[7,10] In our experience with chorionic gonadotropin, pregnant mare serum and rat anterior pituitary injected for 20 days, we have seen x-zone disappearance in the female, but not as a consistent response, despite uniform treatment. Pfeiffer and Hooker[11] have studied the source of ovarian androgen in the mouse and obtained androgen release under pregnant mare serum stimulation. Thus, ovarian androgen might be responsible for x-zone disappearance under gonadotrophin stimulation. Argyris in our laboratory, in preliminary studies, has found an increase in adrenal alkaline phosphatase along with x-zone disappearance when chorionic gonadotrophin was injected for 20 days. The increase in enzyme was not seen if the x-zone remained, despite identical treatment.

Since the androgen-adrenal x-zone relationship is tenable, experiments involving the reproductive system have included a study of the adrenal as a measure of hormone secretion in addition to seminal vesicle weight. For example, injection of 2 μg. of stilbestrol daily for 16 days into 24-day-old male mice results in suppression of the reproductive system and retention of the x-zone. Similarly treated mice were then studied at 10, 20, and 30 days after the last injection to observe the rate of recovery. The testes contained spermatozoa 20 days after the last stilbestrol injection, and seminal vesicle weight was normal, but the x-zone was still present. Ten days later, the x-zone had also disappeared (TABLE 3). Normally, spermatozoan appearance occurs at the same time as x-zone disappearance.

TABLE 3

Recovery of 24 day old male mice following 0.002 mg. stilbestrol × 16

Treatment	*No.*	*Testis*	*Seminal vesicles*	*Adrenal*	*x-zone*
S + 10 days	7	83	18	7.9	+
Control	6	178	116	5.7	–
S + 20 days*	7	144	114	5.0	+, –
Control	7	183	117	4.2	–
S + 30 days	7	180	220	4.5	–
Control	6	200	176	4.0	–

* 5 I.U. PMS daily in recovery period hastens x-zone disappearance.

The relationship of age and hormone responsiveness has been of interest to us for some time and, again, the adrenal x-zone has served as an adjunct to studies on the reproductive system. The responsiveness of mice 5, 10, 15, and 20 days old to a single subcutaneous injection of 50 I.U. of pregnant mare serum (PMS) was determined five days later. In the male, seminal vesicle weight responded maximally in mice 10 days old at injection. At this age and older, the adrenal x-zone disappeared as the testis steroid secretion was prematurely augmented.[12] The five day old male, too, responded to PMS as indicated by seminal vesicle weight increase, but the adrenal x-zone was not influenced at 5 to 15 days after one injection, suggesting that the tissue cannot respond at this age or that insufficient androgen was released. In the female, the reproductive system responded at all above mentioned ages, but the x-zone remained intact.

A critical age in rat development seems to exist approximately 10 days postnatally. Merklin[13] has found that as little as 1 μg. of estradiol dipropionate injected into 10 day old mice as a single injection will clearly delay the appearance of spermatozoa in the testis and that adrenal x-zone disappearance is related to recovery of testis function.

Ten day old mice of both sexes have also been studied after the single subcutaneous injection of 1 mg. of testosterone propionate. A study of the reproductive system and adrenal was made at 10, 20, and 30 days after the injection. In male mice, the androgen delayed the onset of spermatogenesis slightly, but full recovery was observed in 30 days. The x-zone was removed by the testosterone injection and the subsequent recovery of the reproductive system kept a continuous androgen outpouring so that it did not return. In the female, the reproductive system was retarded and the x-zone was lacking for 20 days after injection, but, 30 days after hormone treatment, the x-zone had reappeared (TABLE 4).

Other studies have related brown degeneration in the mouse adrenal to tumors.[7,14] It was deemed of interest to study the female mouse adrenal in precancerous periods for possible adrenal changes. The experiment involved feeding 0.1 per cent 2-acetylaminofluorene in a semisynthetic diet containing 24 per cent casein for periods of 45 and 100 days, during which time hepatic tumors did not form. Adrenal histology was normal in all mice examined at the 45 day period, but, at 106 days, the adrenal x-zone in most of the control mice had small areas of vacuolization suggestive of beginning degeneration. No x-zone degeneration was observed in the adrenals from mice fed the carcinogen. The adrenals were also studied for alkaline phosphatase, which, in the normal, is distributed in the capsule, zona glomerulosa and in capillaries, and the drug fed animals exhibited this normal pattern. The control adrenal glands exhibited a definite alkaline phosphatase concentration in the x-zone, specifically, in the nuclei of degenerating cells. Since the factors inciting degeneration of the x-zone in female mice of 80 to 200 days are not known, the effect of carcinogen feeding is not explainable at this time.

TABLE 4
Testosterone propionate (1 mg.) given in one injection to 10 day old mice

Treatment	*Testis*	*Seminal vesicles*	*Ovary*	*Uterus*	*Adrenal x-zone*	
					♂	♀
T.P. + 10 days	42	21	3.0	14	−	−
Control	65	6	3.4	13	+	+
T.P. + 20 days	99	37	3.6	43	−	−
Control	134	27	4.2	35	−	+
T.P. + 30 days	147	92	6.0	66	−	+
Control	159	100	6.0	74	−	+

+ = present
− = absent

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SECTION OF PSYCHOLOGY

PSYCHOMOTOR FUNCTION AS CORRELATED WITH BODY MECHANICS AND POSTURE

By LULU E. SWEIGARD*

The term psychomotor, separated into its parts, means mind and movement. It suggests that activity of the mind is accompanied by movement in and of the human body. Movement, initiated in the body itself, is produced by the action of muscles, mainly on bones. Hence, we may say that activity of the mind is accompanied by muscle action, or that thinking has its motor components. Experimental work substantiating such statements has been done mainly in the field of Psychology, since the time of William James, who first placed emphasis on the theory.

Because of preconceived and frequently inaccurate ideas regarding body mechanics and posture, especially posture, the meaning of each, as used here, must be defined. Consider body mechanics first. Physics defines mechanics as that branch which treats of the motions of bodies and the causes of changes in these motions. Body mechanics, then, must be defined, similarly, as treating of the movement of the human body and the causes of changes in its movement. Causes of changes in motion of the human body are no different from the causes of changes in motion of inanimate bodies. Hence, we cannot logically escape the fact that physical laws apply to the balance and movement of the human body, just as to any other structure. Failure to recognize this fact has led to illogical and unscientific reasoning regarding body alignment and movement. Further, it has decreased the value of a number of experimental studies by its interference with a scientific approach to problems.

Consider next the term, posture. Some authors speak of "postures," thus implying that each individual can have more than one posture. In view of the results of experimental work, this is not true. Perhaps the erroneous idea that a person can have "postures" is due to failure to distinguish between the two terms, posture and position. Position means the manner of being placed or arranged, as in standing, sitting, or stooping. Posture, in contrast, means the *relative* position of the various parts of anything. In the human body it is the relative position, or pattern of relationship, of the parts of the body. A pattern of relationship of body parts, or posture, tends to exist in each and every position the body may assume.

The meaning of posture may be clarified further by turning to the results of research on body alignment. One study was made by means of radiographs of the skeletal framework in the standing position; another study was made by means of photographs of the nude body, likewise in the standing position. Each of these studies indicates: (1) that in every individual there is a consistent and persistent pattern of relationship in the alignment of the various parts of the body as its equilibrium is maintained in the upright position; (2) that there is similarity in the pattern of alignment of the majority of individuals; and (3) that the nearer the parts of the body are to its center of gravity, the less variable the pattern of relationship of parts tends to be. The latter is logical from the standpoint of physical laws.

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The posture pattern also influences the manner in which all movement activities are performed. To some extent this can be verified by observation. Characteristic postural idiosyncracies are noticeable in friends whether they are standing, sitting, or moving. The degree of influence, however, of the postural pattern on movement must be determined through experimental study. One such study of the influence of the posture pattern on the movement of walking will be completed in the near future.

Having defined body mechanics and posture, there is need for consideration of *how* ability to maintain equilibrium and to move is established and *why* patterns of postural alignment and movement tend to be consistent and persistent, even to grow worse. The human being is a self-directing mechanism through the action of his muscular system on his skeletal framework. Muscles, however, do not contract to produce movement unless and until they receive motor messages from the central nervous system. How are these messages instigated? What determines their muscle destiny? Is the motivating force a thought or idea? If so, is the thought concerned with muscle action; or is it concerned with movement only?

To answer such questions briefly, the development of ability to move must be considered. The body is the only means of expressing one's self. For expression there must be movement. Expression is sought from birth, and movement, to accomplish this, must be learned slowly by trial and error methods until adequate muscle coordination is established.

The individual is born with certain primary reflexes which he is unable to control, or change but briefly, if at all. Beyond primary reflexes, other reflexes make it possible at birth to move arms and legs and to squirm the trunk, but without specific direction. Neuro-muscular coordination toward directed and effective movement has not yet been established. But it can be and is, in the early years of life. The learning process takes place through building S-R bonds—muscle responses to specific stimuli. These are slowly established and, more frequently than not, the initial motivating force which activates many and complicated S-R bonds is the idea or thought of movement. Thus neuro-muscular action patterns are conditioned so that movement can be directed effectively toward the accomplishment of purpose.

Many influences play on this conditioning process: inherited structure, especially skeletal, muscular, neurological; mechanical, such as toys, furniture, clothing; pathological, as disease or sickness of any kind; injury, even though it thwarts only temporarily the ability to move; nutrition, with its general effects on growth and health; sociological, in its adjustment to others of society; and psychological, which are of great variety. Within the realm of psychological influences occur some of the most detrimental. The child imitates the posture and movement of those he admires and loves. He unconsciously absorbs and tends to live imposed ideas about posture and movement, whether they are good or not. He is influenced by the emotional stability of his parents, teachers and others about him. Discipline, or its lack, also has an important influence on the degree of muscular strain created in his body.

Thus, through trial and error methods, influenced by innumerable factors, neuromuscular reflexes concerned with balance and movement of the body are conditioned;

they become a matter of habit. Habits exist in the neural pathways traveled by motor messages through and from the central nervous system to the muscles. Continued use over the years sets more firmly the habit patterns of neuro-muscular action. Beyond their use required by the usual activities of life, few people are fortunate enough to escape the habit-setting effects of time-honored postural admonitions and of exercises for different parts of the body. As they are practiced, conditioned reflexes are used, habits are intensified, and muscular strain is added.

It may be concluded then that conditioned neuro-muscular action accounts for and is the *main cause* of the consistence and persistence of a postural pattern and manner of movement; also, that it limits both the ability to assume a good posture at will and the ability that may be attained in the performance of a skill, whether this skill be one used in work, play, or recreation.

With an understanding of the reason for postural pattern and manner of movement, attention can be turned to the possibility of their change, whether change is worth while, and how it can be brought about.

No one can deny the fact that that which has been conditioned in the human being can be reconditioned. Just as surely as Pavlov conditioned dogs so that their mouths watered at the sound of a bell, so can the human being be reconditioned in his muscular responses to stimuli. Toward what end, is the important consideration.

To determine logical and worth-while goals in the reconditioning process, reasoning should proceed along mechanical lines. The established goal which has led to the construction of increasingly better inanimate mechanisms is a greater degree of efficiency—a better ratio of useful work to energy used. Granting that this is a worthy goal in engineering, economically and otherwise, the important factors influencing efficiency in inanimate structures should be noted. They are, primarily, a well-balanced supporting framework, ease of movement of parts of the structure in the performance of their work, and movement of all parts in accord with physical laws and principles of mechanics.

The fundamental aim in education and training for better body mechanics and posture likewise should be a greater degree of efficiency. This aim cannot be attained in the function of the human body without consideration of the same primary factors which influence the degree of efficiency in other mechanisms. In terms of the human body they are: alignment of the supporting skeletal framework in accord with principles of balance; flexibility of all joints in accord with their architecture and with principles of muscle function; and good mechanical use of all parts of the body, especially of the upper and lower extremities.

Before any procedure can be employed effectively to increase efficiency in the operation of the human mechanism, one must have (1) a clear concept of *ideal* body alignment, and (2) knowledge of deviations from the ideal which are typical of the majority of individuals.

To consider the ideal first, alignment of the skeletal structure would conform in so far as its architecture would allow, with the simple principles of balance. These principles are concerned with the adequacy of the various bases for support of weight throughout the structure, and the distribution of the weight of structure so that it is near the central line and near the base. Conformity with these principles determines the degree of stability of equilibrium. Logically, the true base of the human structure is the pelvis, since the center of gravity is located in that area. Recognition of this is essential to scientific reasoning in regard to both body alignment and movement.

As the concept of good skeletal relationships and alignment is built, accompanying muscle action and body contour can be studied and interpreted in terms of the internal framework. The latter is simple as a statement; actually, it is difficult and complicated—never ending and never fully known. It must be taken into consideration, too, that structural builds are different;

hence, body contours are different, even when bones are well balanced. Esthetic preference of a type of human figure must not be allowed to confuse scientific thinking.

Study of the deviations from ideal alignment indicates that there is, in general, a typically poor posture pattern found in the majority of individuals. The familiar postural difficulties, as forward head, round back, prominent abdomen, etc., should be interpreted in terms of poor skeletal relationships. In the majority of individuals, to a greater or less degree, interpretation shows that the following faulty relationships tend to occur. The trunk is too short in back, too long in front, and asymmetrical laterally. The lower extremities assume a backward and laterally different slant toward the feet. There are few, if any, spinal columns without lateral deviations of more or less degree, and few whose natural forward-backward curves are well balanced for weight support. Both the ribs and the shoulders are invariably supported at a higher level from the pelvic base than their bony attachments indicate. Finally, but not to complete the picture, the head tends to be too far forward, as well as rotated and tipped laterally.

All these deviations from good alignment imply a pattern of overwork, or hypertonicity, in muscle action. It must be so, since individuals, by means of muscle action, maintain their equilibrium in a consistently poor pattern of alignment.

As a concept of typically poor skeletal alignment is built, relative muscle action and distortion of body contour occurring with it must be interpreted. There are a few experimental studies which aid in this. They do not give a solution, however, to the intricate problem of muscle action; they serve mainly to point the way to better reasoning on a problem which seems unending in its complication.

Posture teachers are indebted, primarily, to neurologists and psychologists for the scientific support of changes now occurring in their teaching. Such changes are supported by the principle: thinking influences muscle action. It can influence it detrimentally or beneficially. Its detrimental influence is within the experience of most people, notably that of continuing the work and battles of the day after going to bed at night, thus building greater strain in the body instead of releasing it. Its beneficial influence is demonstrated especially in the work of Mabel Elsworth Todd* and Sister Kenny, and to some degree in the work of many others in both Medicine and Education.

Mental activity, then, especially that in which the thought of movement is involved, is the most effective means employed thus far to recondition neuro-muscular action patterns in the body. Such mental activity can be encouraged in a variety of ways. The key to each, however, is mental imagery, that is, visualization of an imaginary situation. Teaching can and should proceed in a manner which will lead the student to think in terms of concrete images. As this is achieved, image-directed muscular responses tend to occur. When no voluntary aid is given by the subject, concentration on a mental image promotes a coordination of muscle action whose resultant is the line of action of forces at play in the imagined situation.

In order to obtain the best results, especially when working with a group, the teacher should first help students gain the ability to change muscle action through concentration on mental imagery, without giving voluntary aid, and to recognize and interpret the kinesthetic experience accompanying such change. As this ability is attained, voluntary movement may be added to promote greater power in the newer muscle patterns and to speed up change. Since time does not allow detailed consideration of movement, suffice it to say that it must be carefully chosen, and that its effectiveness is controlled to a large extent by simultaneous use of appropriate mental imagery.

Of great importance to effective results in teaching are the answers to the following questions. What mental images shall be used, where in the body, and what shall be the direction of visualized movement? The answers are determined from the typical deviations of skeletal structure from ideal alignment, and from the principles governing muscle action.

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* Todd. M. E. *The Thinking Body.* Paul B. Hoeber, New York.

Through comparison of typically-poor with ideal skeletal relationships the writer determined eight "lines-of-action" needed to produce better body alignment and greater freedom of movement. Each line-of-action extends from one part of the skeleton to another. Along each line, distance between bones either increases or decreases. Such change is promoted by visualized action, without voluntary movement. The mental images on which the individual concentrates to promote the line-of-action are so designed that the resultant of the action in the image will be identical with the desired line-of-action. It may require recoordination of as many as fifty or one hundred muscles to produce this resultant.

Briefly, the lines-of-action, stated according to their purpose and direction of movement, are as follows: (1) to lengthen the back downward; (2) to widen across the back of the pelvis between the great trochanters of the thighs; (3) to narrow across the front of the pelvis; (4) to produce alignment of the knee with the thigh joint; (5) to shorten the inside of the foot toward the heel; (6) to narrow the rib-case; (7) to shorten from the midfront of the pelvis to the top of the forward lumbar curve; and (8) to shorten (or lengthen) from the top of the breast bone to the top of the forward cervical curve.

Thus, the lines-of-action determine the location and direction of movement in the body, which is only to be visualized. Images that may be used to promote lines-of-action are many and variable. However, they should seldom, if ever, be created in terms of muscle action which is so highly complicated. Furthermore, the student does not need actually to know a thing about muscles to change his posture and movement.

Presenting movement to be visualized is not the only way to promote thinking in terms of mental images. Another way lies in the educational procedure itself—the presentation of pertinent facts in an interesting and challenging way, leading the student to think in terms of concrete imagery in his own body. It should be remembered, also, that people tend to be visual, kinesthetic, or auditory minded. This requires some modification in procedure to make it equally effective for all.

To summarize the primary qualifications of one who might be termed an engineer of the human body, they include the following: a clear mental picture of ideal skeletal alignment, supported by knowledge of physical laws and principles of mechanics; ability to visualize within body contour the existing faulty alignment of bones and to interpret the interrelationship between this and existing muscle action; knowledge of change needed in bony relationships to promote greater structural stability and freedom of movement; and finally, logical aims by which proposed change in sleletal alignment may be evaluated.

To avoid possible misinterpretation of some of the statements that have been made, it seems advisable, before completing this discussion, to clarify the difference between two terms, efficiency and relaxation, which too often are used interchangeably.

Dr. Edmund Jacobson, in his report*, states "muscular relaxation (conceived as a limit) can be regarded as freedom from effort, physical and mental." If this definition is accepted, there is only one way to attain relaxation to the greatest degree, and that is during rest of the body in a specified lying position and with minimum mental activity. Increased efficiency, on the other hand, means the accomplishment of purpose in any activity and in any position with the least expenditure of energy.

To increase the degree of relaxation, muscle work is decreased, but it is not recoordinated for the purpose of changing skeletal relationships. Hence, relaxation would tend to occur in accord with established patterns of neuro-muscular action, changing them but slightly, if at all. Whatever change may occur in muscle coordination will be undirected and in response to the need to maintain equilibrium and to move while trying to relax. Such automatic and undirected change may be for the better; or it may be for the worse, especially in certain postural deviations, such as lateral curvature of the spine or sacroiliac strain.

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* Jacobson, E. Theory of hypertension in man. Trans. N. Y. Acad. Sci 11(2): 49-50. 1948.

To increase the degree of muscular efficiency, in contrast to relaxation, there must be better alignment of the skeletal framework. This is attained only as muscle action is reconditioned to produce it. As the mechanics becomes better, the total result is less muscle work.

It seems logical then, to state that relaxation means less work in muscles. But it is not necessarily accompanied by an increased degree of efficiency, even though the metabolic rate may be lowered. On the other hand, an increased degree of efficiency means neuro-muscular recoordination, and it *is* accompanied by less muscle work.

To summarize, the development of the subject has dealt with: (1) the meaning of the terms, psychomotor, body mechanics, and posture; (2) the significance of conditioned reflexes in the self-direction of the human mechanism; and (3) how conditioned reflexes may be reconditioned toward the production of those mechanical considerations which promote decrease of strain in all human activities.

DIVISION OF MYCOLOGY

HISTOPLASMOSIS IN ANIMALS

C. W. EMMONS*

Much attention has been given to the special public health problems presented by bacterial diseases which affect both man and animals, but relatively little is known about this aspect of fungous diseases. Histoplasmosis, blastomycosis, sporotrichosis, coccidioidomycosis, and actinomycosis are some of the more important systemic mycoses which are common to man and animals. Accurate knowledge about their frequency and the possibility of their transmission from animals to man is fragmentary, although inspection of slaughtered animals in packing plants and scattered reports in medical and veterinary literature give some information about the occurrence in animals of the last two mentioned.[29] Coccidioidomycosis is known to occur also in rodents.[7]

Histoplasmosis in animals presents some unusual features, which deserve further consideration because of their intrinsic interest and because the study of histoplasmosis in animals promises to elucidate some of the puzzling problems in the epidemiology of the human disease. It should be pointed out at first that human and animal histoplasmosis have an identical etiology; both are caused by *Histoplasma capsulatum.* Strain differences are found among the isolates of the fungus, but these differences are not correlated with a particular host source. Fifteen strains recently isolated from animals in southern Georgia[12] were alike in one peculiar characteristic, which, however, was not seen in strains isolated from animals in Virginia but has been seen in a few human strains. This example serves to illustrate the significant point that *Histoplasma capsulatum*, like other fungi, varies or mutates freely. As increasing numbers of strains are available for study, it becomes apparent that, despite minor differences, one species, *H. capsulatum*, causes both human and animal histoplasmosis.

The study of histoplasmosis in animals is especially important at the present time, because of the current interest in human histoplasmosis. Proved histoplasmosis in man is a rare, almost invariably fatal disease, and only some 125 cases have been reported. There is a keen interest in the disease at the present time because the use of the histoplasmin skin test has shown that in some geographic areas as many as 80% of the general population react to this test.[3, 19, 21, 23]

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In some areas there is a high correlation between histoplasmin sensitivity and the occurrence of benign, nontuberculous, pulmonary calcification, which some investigators believe is a hitherto unrecognized form of histoplasmosis. Unfortunately, the histoplasmin skin test is not specific.[13] Clinical and laboratory confirmation in the diagnosis of a possible, very prevalent, benign histoplasmosis has not been obtained to date.

In the United States, the geographical areas of high histoplasmin sensitivity and of benign, nontuberculous, pulmonary calcification in humans coincide rather closely[3,21,22] and are limited, according to reported surveys, principally to a belt extending from eastern Kansas to the Atlantic coastal plain and from the middle of Iowa and northern Ohio southward through Tennessee. This sharp geographical limitation does not correspond to the known distribution of proved histoplasmosis. Although most of the reported cases have occurred in eastern United States, sporadic cases have occurred almost throughout the world, and it is not clear to what extent the distribution of the 125–150 human cases reflects either a true picture of the disease or medical interest in it and the utilization of laboratory facilities in its diagnosis.

A more precise knowledge of the actual distribution of histoplasmosis would aid in the interpretation of the equivocal histoplasmin skin test and in an understanding of the nature of benign, nontuberculous, pulmonary calcification. The detection of a sufficiently large number of human cases of proved histoplasmosis, even at the rate at which they have been reported during the past three or four years, does not offer an early answer to the questions raised. A systematic search for histoplasmosis in animals, especially in the domestic rat, does provide a practicable and conclusive method of demonstrating histoplasmosis in a given area. In our studies at the National Institutes of Health since November 1946, we have isolated *H. capsulatum* in culture from 48 naturally infected animals,[9, 10, 11, 12, 19, 20] and this probably equals the total number of strains of the fungus which have been isolated from human cases since DeMonbreun's classic studies 15 years ago. Application of the knowledge that the feral rat is susceptible to histoplasmosis and that naturally acquired histoplasmosis in the rat can be proved by the isolation of the fungus from the liver and spleen has already yielded important new information about the disease. Demonstration of histoplasmosis in rats or other animals provides the only practicable method now available for determining the geographical distribution of histoplasmosis.

I shall mention only briefly experimental histoplasmosis in laboratory animals. After DeMonbreun[5] isolated the fungus from a human case he reproduced the disease in monkeys and reported that puppies and mice were less susceptible. He subsequently[6] observed a natural infection in the dog and produced experimental infection in that animal. Hansmann and Schenken[14] reported experimental infection of the dog, rat, rabbit, and guinea pig. Parsons[24] described in considerable detail experimental histoplasmosis in mice. He observed generalized infection in mice when the mycelial form of the fungus was injected intravenously, but found only localized lesions when it was injected intraperitoneally. This has not been generally true in later investigations, and it may be that the use of a poorly sporulating strain was responsible for the result Parsons reported. The white mouse is a susceptible and satisfactory laboratory animal and develops a fatal infection when heavily inoculated with a virulent strain. When mice are given a small inoculum, they may develop a non-fatal form of the disease. This development of chronic histoplasmosis is notably evident, in my experience, in the rat and guinea pig. I have isolated *H. capsulatum* from guinea pigs two and one-half years after inoculation.[11] When killed, the animals appeared healthy and had no gross lesions at autopsy. By infecting a series of guinea pigs by intraperitoneal injection of a uniform dose of conidia and killing the animals at suitable intervals, one can reconstruct the following sequence of events. The inoculum is quickly enveloped, and within 10–15 days a hard, tumor-like mass involving the omentum and pancreas develops. The original conidia and hyphal fragments of the inoculum can be found within abscesses in this mass. The liver and spleen become enlarged and the animal develops a systemic infection. Within a month, typically, the peritoneal lesions begin to regress, the liver and spleen return to normal size, and within a few weeks no gross evidence of infection remains; although *Histoplasma* can still be recovered in culture from the spleen and frequently also from the liver. The type of infection found in naturally infected wild animals appears to be similar to this chronic form developed in many experimentally infected animals.[10]

Historical. Naturally acquired histoplasmosis was unproved prior to 1947 in any animal except the dog. There are two early papers in which fungus infections resembling histoplasmosis were reported in mice and rats. The histopathological appearance was compatible with histoplasmosis, but the descriptions of the fungus were incomplete, notably as to cultures. Sangiorgi[26] described a rat captured in the laboratory, in which he found in the mononuclear cells of the enlarged spleen a fungus which he called a *Cryptococcus*. The description closely resembles that of the intracellular form of *Histoplasma*. It was reported that after 48 hours' incubation there developed in the cultures a *Cryptococcus*, which, when injected intraperitoneally, failed to produce disease in a guinea pig. No description of a mycelial type of growth was given.

Shortt[28] described, in 1 rat and 7 mice experimentally injected with flagellates, a fungus which, he stated, resembled Darling's *Histoplasma*, but which he named *Cryptococcus muri*. No cultures were described. More recently, Levine *et al.*[16] reported in a ferret a disease which may have been histoplasmosis, but cultures were not obtained. In the absence of data on cultures and in view of the writer's frequent isolation of a *Cryptococcus* from mice and rats,[9] some doubt exists regarding the actual nature of these fungi.

Dog. The dog was the first animal in which naturally acquired histoplasmosis was proved by adequate study and by the isolation of *H. capsulatum* in culture. The case, already mentioned, was reported very completely by DeMonbreun.[6] Thirteen cases of canine histoplasmosis have been reported since DeMonbreun's paper appeared,[1, 2, 19, 23, 27, 30, 31] and several additional unreported cases are known to be under study. The infected animals varied in age from 1 to 7 years. There were 7 males and 3 females among those whose sex was stated. Breeds represented were Boston bull, fox hound, Springer spaniel, Boston terrier, pekingese, mongrel, fox terrier, and English bull terrier. These animals came from Nashville, St. Louis, Des Moines, Brazil, Panama Canal Zone, Kansas, and northern Virginia. Tomlinson and Grocott[31] point out that their canine case from the Canal Zone represented the first demonstration of *Histoplasma* in that area since Darling's original 3 human cases 40 years before, although 15,000 autopsies had been done during the intervening period. Clinical and pathological findings were varied, but most of the dogs showed loss of appetite, emaciation, diarrhea, and enlargement of spleen, liver, and mesenteric nodes.

It is not possible to tell from the literature on canine histoplasmosis the probable source of infection. The cases reported have been sporadic and unrelated to each other, except for two litter mates reported by Harmon and those reported from northern Virginia, where two dogs from one litter and their sire had histoplasmosis, as proved by sections and cultures.[19, 27] Two other dogs from this litter died with undiagnosed diseases, and, although material was not available for study, it was believed that these dogs may have had histoplasmosis. The possibility that dogs may be infected from rats which they catch must be considered.

Mouse. Up to the present time, the isolation of Histoplasma from feral rodents has been reported only from our laboratory in the National Institutes of Health. Since November 1945, a small scale but persistent and more or less systematic attempt has been made to gain additional knowledge about histoplasmosis by a search for animal hosts of the disease. This search has been conducted in Virginia, Maryland, District of Columbia, Iowa, and Georgia. Attention has been directed at one time or another toward several species of mice, including the meadow vole (*Microtus spp.*), white footed mouse (*Peromyscus spp.*), shrew (*Blarina brevicaudata*), and pine mouse (*Pitymys pinetorum*). The reasons for selecting these species for study have not as yet been justified by results. However, among more than 1000 house mice (*Mus musculus*) trapped, one animal with proved histoplasmosis was found.[19] This was trapped at a house where one of the infected dogs mentioned above had lived more than a year before. No gross lesions were observed when this mouse was killed. Histologic sections showed only microscopic epithelioid granulomata in lung, liver and spleen, and a few fungus cells.

Rat. The search for histoplasmosis in the rat has been more successful in revealing an animal reservoir of histoplasmosis. Twenty-three brown rats (*Rattus norvegicus*) with proved histoplasmosis, representing slightly more than 2% of those trapped, have been found in Loudoun County, Virginia.[9, 10, 11] These infected animals have been captured on several widely separated farms in this rural county. No importance is attached to this percentage, since no attempts were made to take a random sample. When infected rats were once found on a farm premise, trapping was continued at that location in an attempt to learn more about the local occurrence of histoplasmosis.

Rats were captured in steel traps, brought to the laboratory alive when possible, and the autopsy was performed promptly after the animal was killed. Sterility precautions were observed in the autopsy, since the presence of bacterial or fungus contaminants frequently inhibit growth of Histoplasma. Cultures were made from liver, spleen, adrenal, urinary bladder, and lung by streaking a piece of tissue on a Sabouraud agar slant. The cultures were incubated at 30°C for 3 or 4 weeks before they were discarded as negative.

No correlation between gross lesions observed at autopsy and the isolation of *Histoplasma* was observed. In most of the infected rats the spleen and liver were normal in size and appearance. Histopathologic studies of these and other organs involved showed only microscopic, epithelioid cell granulomata.[10] *Histoplasma* was found in small numbers in some infected rats and not at all in a few, although microscopic lesions were present. So far as could be determined from the infected animals examined, histoplasmosis is a benign infection in the brown rat. This is in agreement with observations of histoplasmosis in the laboratory strain of white rat used in experimental studies of histoplasmosis.[9]

The search for histoplasmosis in the rat was extended to south Georgia where, in collaboration with the Typhus Investigations unit of the Communicable Disease Center, U.S.P.H.S., some 1300 rats have been cultured.[12] These studies resulted in the isolation of *Histoplasma* from 7 brown rats and 4 roof rats (*R. rattus*). The strains of *H. capsulatum* isolated from Georgia rats differ in one respect from those isolated in Virginia. They produce enormous numbers of microconidia and relatively few macroconidia. In this respect they resemble a few strains isolated from man, notably those described by Negroni[18] from South American cases. Further study may reveal important geographic differences in strains of *Histoplasma*, but no adequate reason appears at the present time for splitting the species *H. capsulatum* on the basis of differences observed among these rodent strains.

Cat. The demonstration of histoplasmosis in the rat suggested at once an examination of the cat, since many farm cats are rat-catchers. During the past two years, we have killed and made cultures from 49 cats collected in Loudoun County, Virginia. Five of these had histoplasmosis, as proved by culture.[19] The first infected cat was obtained in July, 1948. Three of those with histoplasmosis came from a farm where 8 of 124 rats previously trapped had this disease. Thus, it seems probable that the cat is infected by eating rats with histoplasmosis.

A surprising feature of histoplasmosis in the cat is the limited extent of the lesions. Histoplasma was isolated from the spleen in only one animal. In the other four the only isolation was from one or more of the cervical lymph nodes.

There is one previous report of histoplasmosis in the cat, but, as Meleney[17] has pointed out, this was an erroneous diagnosis for toxoplasmosis.

Skunk. The highest incidence of infected animals was found in the spotted skunk (*Spilogale putorius*). Five of six skunks trapped and cultured in southern Georgia were infected.[12] Isolations were from spleen and liver in these animals. The skunk is a carnivore and is known to eat rats. A further study of this animal and of other related species is indicated.

Horse. There is one report of histoplasmosis in the horse.[25] The diagnosis was made entirely by identification of the fungus in sections of lung tissue. Since the fungus causing epizootic lymphangitis in horses is indistinguishable from *H. capsulatum* in tissue, some doubt might be entertained concerning the correctness of the diagnosis.

In reviewing the status of proved histoplasmosis in animals, the dog may be listed first because of the chronological order of reports, the recognition of canine histoplasmosis independently by several investigators, and the intimate association between man and the dog. Our studies have first proved the occurrence of histoplasmosis in *Mus musculus*,[19] *Rattus norvegicus*,[11] *R. rattus*,[12] *Felis domestica*,[20] and *Spilogale putorius*.[12] In all except the house mouse, *Histoplasma capsulatum* has been isolated from substantial numbers of individuals. All the animals, with the exception of the spotted skunk, come into rather intimate contact with man, his food and clothing, or other objects in his environment. Whether the exposure of man to animals with histoplasmosis is important in the epidemiology of human histoplasmosis is a question of considerable importance. Unfortunately, we are unable to answer it at the present time. The limited extent of infection in most of the animals studied[10] suggests the improbability of direct transmission from animals to man either by ectoparasites or exposure to excreta.

One item of collateral evidence may be pertinent. The demonstration of histoplasmosis in rats stimulated a search for a free-living or saprophytic form of *Histoplasma* in the environment of infected animals. During the past two years, 387 samples of soil or various types of debris have been collected on farms from which infected animals have been collected. By suspending soil in physiological saline and injecting the upper portion of such a suspension into mice, *H. capsulatum* has been isolated from two soil samples.[8]

In addition to the isolation of *Histoplasma* from mice infected from these two soil samples, the typical macroconidia of *H. capsulatum* were visibly demonstrated by microscopic examination of one of these suspensions.[8] Since these macroconidia have never been found developing in the animal body but are known to develop freely in the saprophytic growth cycle of the fungus, their demonstration in soil seems to indicate that there is a free-living form of *Histoplasma* in the environment of man and animals. Therefore, it is not necessary to postulate transmission of histoplasmosis directly from man to man or animal to man in order to explain the occurrence of human histoplasmosis. The possibility exists, however, that the seeding of soil by the eventual death of animals with chronic histoplasmosis is important in maintaining *Histoplasma* in the soil flora.

The significance of an animal reservoir of histoplasmosis is still obscure. The fact that such cosmopolitan animals as the rat, cat, and dog are important hosts of *Histoplasma* may be significant in the world-wide distribution of human histoplasmosis. A comparison with coccidioidomycosis is interesting. Coccidioidomycosis also occurs in the dog, but, among the native wild animals present in the limited endemic area of coccidioidomycosis, only species of *Perognathus*, *Dipodomys*, and *Citellus* peculiar to the desert have so far been shown to be important hosts of *Coccidioides*. A possible necessary relationship to these desert-dwelling rodents is the only explanation which has so far been offered with any supporting evidence to account for the limited geographical distribution of coccidioidomycosis.[7]

To summarize, naturally occurring histoplasmosis has been proved in the dog, brown rat, roof rat, domestic cat, and the spotted skunk by isolation of *H. capsulatum* in culture. The detection of histoplasmosis in these or other animals is the only practicable method now available for determining conclusively the geographic distribution of histoplasmosis.

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SECTION OF ANTHROPOLOGY

EVIDENCE FOR PLEISTOCENE MAN AT LA JOLLA, CALIFORNIA

By GEORGE F. CARTER*

Introduction. Exposed in the seaward face of an alluvial fan now undergoing active destruction through wave attack is evidence that man lived at La Jolla, California, throughout most, if not all, of the period when this fan was being built. This fan is so situated that it is a sensitive recorder of land and sea level relations. It is not only capped by a mature soil, but it contains buried soil profiles from which some temporal significance can be gleaned. From a combined morphological and pedological study of this fan, some insight into the antiquity of man in Southern California can be gained.

The evidence for man consists of hearths, the shells from seafoods, stone flakes produced by man, and one stone used for the grinding of seeds (technically known as a mano). This material is distributed as follows: hearths, flakes, and shell are to be found throughout the 20-foot thick exposed depth of the fan; the mano was found at a depth of five feet. Careful study of the fan has shown that the depth of burial is not particularly significant, since the fan has been subjected to past periods of erosion and deposition, and some of the deeper materials are younger than some of the higher materials.

Morphology of the Scripps Fan. This fan is now being destroyed by the sea, and its truncated face forms a low cliff 20 to 30 feet high. It is probably significant that the fan has been under rapid retreat in the present period of rapidly rising sea level. At any rate, the fact that the alluvial fan is being *destroyed* under the present conditions indicates that it could not have been *built* under the existing conditions. Both the submarine contours off this beach and a projection of the remnant of the fan indicate that when it was built it must have extended approximately one half mile seaward of its present terminus. The depth of water indicated at the probable outer edge of the fan would then be at 100 feet. This may be taken as an approximation of the minimum amount of lowering of the sea in relation to the land to allow the fan to grow.

The evidence for the origin of the fan may then be stated as follows: At a time when the sea stood in relation to the land 100 feet or more lower than at the present, a gently sloping platform approximately three quarters of a mile wide (one half mile now under sea, plus the one quarter mile still above sea level) was exposed at the foot of the 300 cliff terminating the seaward side of the Linda Vista Mesa. Alluvium eroded from a portion of this mesa was deposited where a small canyon opened onto this shelf, and a sharp reduction of gradient occurred. Under these conditions, an alluvial fan was constructed. With a subsequent rise of sea level in relation to the land, the unconsolidated alluvium has been exposed to wave attack and has retreated approximately one half mile.

Dissection of the Fan. The fan is now cut by a deep gully. This has resulted both from the cliffing due to truncation and the associated steepening of the gradient of the fan. The face of the cliff reveals marked horizontal bands of differing sedimentary materials. Close study reveals that these bands are not continuous but that in the past they have been cut by gullies comparable to the existing one. These old gullies, however, had been filled and the fan resumed its growth. Careful mapping of the gullies and the principal strata reveals that there have been three major periods of alluviation and of gully-cutting. In terms of the mechanics of accumulation versus truncation and gully-cutting, this sequence of events would seem to call for three cycles of low sea (or high land) level followed by high sea (or low land) level.

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* The Johns Hopkins University, Baltimore, Maryland This paper, illustrated with lantern slides, was presented before the meeting of the Section on April 25, 1949

Fire removal of the protective cover of the fan and its catchment source, extreme floods, and minor climatic fluctuations cannot be arbitrarily ruled out as possible causes of the sequence of gullying and erosions that occurred on this alluvial fan. All of them are theoretically possible causes. On the other hand, the mechanism of fluctuating land or sea level would not only theoretically produce the result, but, under conditions of present rising sea levels, *is* producing results comparable to what has occurred in the past. Lacking any other clear mechanism to produce the observed gullying and filling, and possessing evidence that the present gullying is correlated with a rising water level, it seems logical to incline to the theory of gullying and alluviating as resulting from alternating land-sea level relationships.

Eustatically Fluctuating Sea Level and the La Jolla Fan. The question of land or sea stability is capable, in part, of simple solution. The sea level is known to have changed during the Pleistocene with the addition and subtraction of water due to the melt or accumulation of ice. The sea level is now rising due to the addition of melt water. Sea level during a glacial period can never have been fixed, for the glaciers are known to have waxed and waned within each glacial period. Since we are in the terminal part of a glacial period, the sea level cannot be stable now, nor can it have been in the glacial past. The answer to our question of sea or land movement or both must then be: the sea level has surely been fluctuating; the land level may have been. The movement of sea level can be assumed. Changes of land level must be proven. The evidence from this immediate vicinity suggests a relatively stable coast. Data from regions to the north are conflicting. The Ventura and Los Angeles areas have been described as zones of active crustal movement. A recent study of the Santa Barbara region, however, indicates conditions of stable land and fluctuating sea that are closely comparable to the situation in San Diego county.

In light of the evidence now at hand, the following conclusions seem to be indicated. The low elevation of this fan in an area marked by 25, 60, and 100 foot beaches and terraces indicates that it is younger than these sea stands. The evidence for relative crustal stability in this area throughout at least the upper Pleistocene and the absence of proof for any other mechanism creating this pattern of alternating gullying and filling, taken with the known sea level fluctuation, all combine to suggest that the causal factor in the cycle of gullying and filling may well have been a fluctuating sea level against a relatively stable land mass. If such is the case, the time of the building of this fan must have been during the last eustatically lowered sea level. In terms of glacial chronology, this means the fan grew during Wisconsin times.

Internal Structure and Time. None of the erosions were so extensive as to remove all of the original fan surface. This is evidenced by the ease with which old surfaces can be traced through the length of the face of the fan. These periods of erosion are considered to represent periods of relatively high sea stand and probably to have been the result of truncation and gullying comparable to the present. During each such period of gullying, strongly developed soil profiles developed on the fan surface. The one capping the present surface is described in the soil survey of the region as a mature soil. This implies an age of many thousands of years. The buried soil beneath this is approximately twice as strongly developed. A third such soil profile is associated with the earliest of the major erosions.

If we possessed a reliable figure for post glacial time, we would now be in a position to make some rough computations as to the age of the strata related to these soil horizons. Antevs has estimated that the sea reached within 100 feet of modern levels about 10,000 years ago. Truncation of the fan, with the consequent formation of the modern gully, should date from that time.

An attempt at a time estimate, based on 10,000 years as the assumed onset of truncation, gullying, and initiation of soil formation, is given in TABLE 1. Note that no time has been allowed for gully-cutting, although this process was certainly not instantaneous. Only 5,000 years have been allowed for the filling of the gullies and the deposition of a new layer of material over the fan surface. An allowance of 5,000 years is probably minimal. Consider that the foot of the fan had to be completely rebuilt before the gully could fill and the entire fan surface alluviate. Further, the deposition on this fan, whose surface is large in proportion to its drainage basin, was accomplished by an intermittent stream, and the materials in the fan indicate gentle deposition rather than thick flood deposits. The figure of 40,000 years ago for the beginning of the development of the buried soil profile may, therefore, be a minimum figure. An attempt to date materials associated with this horizon is of interest, since it is in this horizon that evidence for man consists of such positive materials as food, shell, stone flakes, and one mano.

TABLE 1

Geomorphic Cycle	*Suggested Dates*
Present erosion and soil weathering	Began 10,000
Deposition	5,000
Erosion and development of first buried soil	20,000
Deposition of materials, including mano	5,000
Estimated age of mano	40,000 years

The evidence so far, then, suggests that man, possessing the mano and by inference using the metate to grind wild seeds, eating shell fish, and possessed, so far as is now known, of only simple stone flakes, lived at La Jolla something like 40,000 years ago. If the hearths found in deeper strata are indeed evidence of man, then human occupancy of this area extends back through the Wisconsin period. One piece of evidence exists, to be reported elsewhere, for associating man with a 25 foot beach level here. If this coast is proven relatively stable, this would indicate interglacial man and probably double the antiquity suggested above. The poverty of the materials in the alluvial fan do not suggest a people with any traces of a culture suitable to glacial arctic climate survival. This suggests that bearers of this culture entered America via Bering Straits during some milder climatic period.

It should be noted that it is not possible to extrapolate from comparative culture levels to comparable dates elsewhere in America. California, and especially southern and lower California, are regions of extreme cultural lags. Such primitive cultures (in terms of imperishable materials) as we are here dealing with *might* have been contemporaneous with more developed lithic cultures elsewhere in California or America. At this early period, however, such cultural differentiation in the Americas seems to me unlikely.

There seems to be considerable validity to the sequence of lithic industries in this area and their indicated relative antiquity. Despite considerable effort, I have yet to find a developed tool, except for the mano reported here, beneath a comparably developed soil profile. It can be argued that we have simply failed to find a full assemblage of implements. This is possible, but the fact that the site has been under observation for nearly twenty years makes it improbable. Further, other sites in San Diego county beneath similar soil profiles also fail to produce developed implements. Beneath such soils as cap this site, I have yet to find more than unretouched flakes.

A final extension of these findings has considerable interest. If man, of as simple a culture as has here been described, was living on the coast of California at a time when large masses of ice still existed in the northern part of the continent, it raises the question of how he entered America. There is nothing to suggest that he had a boat culture that would enable him to cross the oceans. He must, then, have come by a land bridge. The Bering straits offers almost the only possibility. Since the evidence at La Jolla seems related to lowered sea levels and this to continental ice sheets, the Bering region must have been colder than at present. Any ice free corridor such as has been suggested for the interior of the continent, if only due to the refrigerating effect of the ice masses, must have been greatly colder than the coastal sections of the Bering region. Yet even today, when the arctic is warmer than it could have been during the period of continental ice sheets, it requires a technologically highly developed culture to survive in the Bering region. The evidence at La Jolla, and in San Diego county generally, indicates that technologically advanced cultures are very much later than the material in the site here described.

One might, of course, argue that the La Jolla people are decadent survivals of higher culture. There are, however, no known earlier cultures from which to derive this culture. If there were, even more time would be needed to account for such a change. To explain the phenomena of so primitive a culture in America at so early a time, it seems necessary to assume that its progenitor passed through the Bering strait region at a time when the climate was markedly warmer than at present and when the great continental ice masses did not exist. This is equivalent to suggesting that man entered America during the last interglacial period.

In the past, it was frequently pointed out that the absence of pre-*sapiens* man in America was evidence of the *Recent* (non-*Pleistocene*) time of entry of man into America. Our new knowledge of the antiquity of *sapiens* man invalidates this argument. Such sites as I have reported this evening, together with numerous other reports of primitive cultures suggesting great antiquity for man in America, lead to the conclusion of the probability of man entering America in the last interglacial period.

CONFERENCE HELD

SECTION OF BIOLOGY

"INTERNATIONAL CONFERENCE ON VITAMIN E"

APRIL 14 AND 15, 1949

The Section of Biology held an "International Conference on Vitamin E." Doctor Karl E. Mason, University of Rochester, School of Medicine and Dentistry, Rochester, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, April 15

MORPHOLOGIC LESIONS IN VITAMIN E DEFICIENCY

Morning Session. Chairman, A. M. Pappenheimer, Department of Bacteriology and Immunology, Harvard Medical School, Boston, Mass.

Opening of the Conference, Karl E. Mason.

Introductory Remarks by the Session Chairman, A. M. Pappenheimer.

"Effect of Ovarian Hormones upon

Uterine Pigmentation in Vitamin E-Deficient Rats," by W. B. Atkinson, H. Kaunitz and C. A. Slanetz, Departments of Anatomy, Pathology, and Animal Care, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Histochemistry of Uterine Pigment in Vitamin E-Deficient Rats," by Herbert Elftman, H. Kaunitz and C. A. Slanetz, Departments of Anatomy, Pathology, and Animal Care, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Restoration of the Vaginal Estrus by Alpha-Tocopherol Acetate in Old Rats," by S. Y. P'an, H. Kaunitz and C. A. Slanetz, Departments of Pharmacology, Pathology, and Animal Care, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Increased Tocopherol Requirements During the Rat's Menopause," by H. Kaunitz and C. A. Slanetz, Departments of Pathology and Animal Care, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"On the Biological Assay of Vitamin E by the 'Male Rat Test,' " by Maria Luisa Herraiz and Juan Carlos Radice, National Institute of Nutrition, Buenos Aires, Argentina.

"Vitamin E Studies on Mice with Special Reference to the Distribution and Metabolism of Lipids," by Z. Menschik, M. K. Munk, T. Rogalski, O. Rymaszewski and T. J. Szczesniak, Departments of Anatomy, Polish School of Medicine, Edinburgh, and School of Medicine, University of Ottawa, Ottawa, Canada.

"Ten Year Incidence of Field Encephalomalacia in Chicks and Observations on Its Pathology," by Erwin L. Jungherr, Department of Animal Diseases, Storrs Agricultural Experiment Station, University of Connecticut, Storrs, Conn.

"Central Nervous System Changes in Chronic Vitamin E-Deficient Rats," by Charles N. Luttrell and Karl E. Mason, Department of Anatomy, University of Rochester, School of Medicine and Dentistry, Rochester, N. Y.

Papers Read by Title

"Prolonged Avitaminosis E in Rats and Fibrosis of the Endometrium," by J. Lopes de Faria and J. Dutra de Oliveira, Departments of Pathology and Physiology, Faculty of Medicine, University of Sao Paulo, Sao Paulo, Brazil.

"Ceroid Substance and Its Meaning," by J. Dutra de Oliveira, Department of Physiology, Faculty of Medicine, University of Sao Paulo, Sao Paulo, Brazil.

"The Flourescent Pigment in the Uteri of Rats in Avitaminosis E," by J. C. Radice and Maria Luisa Herraiz, National Institute of Nutrition, Buenos Aires, Argentina.

"The Fluorescent Pigment in Atrophic Testes of Rats in Avitaminosis E," by Enrique Pierangeli, J. C. Radice and Maria Luisa Herraiz, National Institute of Nutrition, Buenos Aires, Argentina.

"The Effects of Hypo- and Hyper-vitaminosis E on Lung Tumor Growth in Mice," by I. R. Telford, Department of Anatomy, School of Dentistry, University of Texas, Houston, Texas.

"The Effect of Chronic Vitamin E Deficiency on the Nervous System of the Rat," by M. Malamud, Marjorie M. Nelson and Herbert M. Evans, Laboratory of the Langley Porter Clinic, San Francisco, and the Institute of Experimental Biology, University of California, Berkeley, Calif.

"Studies on the Histopathology of Vitamin E Deficiency," by Raoul Lecoq and Pierre Isidor, Hospital de Saint-Germain-en-Laye, Paris, France.

"Action of Vitamin E on the Non-Deficient Organism," by Raoul Lecoq, Paul Chauchard and Henriette Mazque, Hospital de Saint-Germain-en-Laye, Paris, France.

"Influence of Tocopherols, Olive Oil, Testosterone and Progesterone on Avitaminosis E Lesions," by Enrique Pierangeli, Domingo Mosto, Maria Luisa Herraiz, Juan Carlos Radice and Francisco J. Cortada, National Institute of Nutrition, Buenos Aires, Argentina.

TOCOPHEROLS AND THEIR ESTERS IN ENZYME AND TISSUE FUNCTIONS

Afternoon Session. Chairman, H. A. Mattill, Department of Biochemistry, College of Medicine, State University of Iowa, Iowa City, Iowa.

Introductory Remarks by the Session Chairman, H. A. Mattill.

"Techniques for Studying the Tocopherols and Their Phosphates in Enzyme Systems," by Stanley R. Ames, Research Laboratories, Distillation Products, Inc., Rochester, N. Y.

"Some Chemical and Enzymic Alterations in Muscles in Experimental Dystrophy," by Charlotte E. Roderuck, Daniel H. Basinski and Mary Alice Barber, Department of Biochemistry, College of Medicine, State University of Iowa, Iowa City, Iowa.

"Alpha - Tocopherol, Alpha - tocopheryl Phosphate, and Phosphorylation Mechanisms," by William M. Govier and Anna J. Gibbons, Department of Pharmacology and Endocrinology, The Upjohn Company, Kalamazoo, Mich.

"The Inhibition of Hyaluronidase by Alpha-Tocopherol Esters," by Wilbur H. Miller and Alice M. Dessert, Chemotherapy Division, Stamford Research Laboratories, American Cyanamid Company, Stamford, Conn.

"On the Effects of Alpha-tocopheryl Phosphate on Oxidative Mechanisms in Striated Muscle," by K. L. Zierler, B. P. Folk, D. Grob, C. Eyzaguirre, L. W. Jarcho and J. L. Lilienthal, Jr., Department of Medicine, The Johns Hopkins Hospital, Baltimore, Maryland.

"Chemical and Biological Studies Related to the Metabolic Function of Vitamin E," by Paul D. Boyer, Division of Biochemistry, University of Minnesota, St. Paul, Minn.

"Relationship of Vitamin E Deficiency to Tissue Peroxides," by Henrik Dam, Department of Biology, Polytechnic Institute, Copenhagen, Denmark.

PROTECTIVE ACTIONS OF VITAMIN E IN CONDITIONS OF METABOLIC STRESS

Evening Session. Chairman, Cosmo G Mackenzie, Department of Biochemistry, Cornell University Medical College, New York, N. Y.

Introductory Remarks by the Session Chairman, Cosmo G. Mackenzie.

"The Significance of Protein in Vitamin E Deficiency," by Thomas Moore, Dunn Nutritional Laboratory, Cambridge, England.

"Comparison of a Fatal Tocopherol-Deficiency Vascular Disease in Rats with the Syndrome Caused by CCl_4 Toxicity," by Edwin L. Hove, Division of Nutrition, Alabama Polytechnic Institute, Auburn, Alabama.

"Dietetic Hepatic Injuries and the Mode of Action of Tocopherol," by Klaus Schwarz, Johannes-Gutenberg University, Mainz, Germany.

"Tocopherol and Hemolysis *in Vivo* and *in Vitro*," by Paul Gyorgy and Catharine S. Rose, Nutritional Service of the Department of Pediatrics, and the Gastro-Intestinal Section of the Medical Clinic, School of Medicine, University of Pennsylvania, Philadelphia, Penna.

Saturday, April 16

PRACTICAL NUTRITIONAL ASPECTS OF VITAMIN E

Morning Session. Chairman, P. L. Harris, Research Laboratories, Distillation Products, Inc., Rochester, N. Y.

Introductory Remarks by the Session Chairman, P. L. Harris.

"The Relation of Vitamin E to Reproduction in Dairy Cattle," by T. W. Gullickson, Department of Dairy Husbandry, University of Minnesota, Minneapolis, Minn.

"Effects of Tocopherols in Vitality of Pigs in Relation to 'Baby Pig Disease,'" by L. E. Carpenter and W. O. Lundberg, University of Minnesota, Minneapolis, Minn., and The Hormel Institute, Austin, Minn.

"Vitamin E Requirement and Economy

in Farm Animals," by J. K. Loosli, Laboratory of Animal Nutrition, Cornell University Agricultural Experiment Station, Ithaca, N. Y.

"Studies on Vitamin E Deficiency in the Monkey," by L. J. Filer, Jr., Ruth E. Rumery, Nan Gan Yu and Karl E. Mason, Departments of Anatomy and Medicine, University of Rochester, School of Medicine and Dentistry, Rochester, N. Y.

"Tocopherols in Food and Tissues," by Mary L. Quaife, Research Laboratories, Distillation Products, Inc., Rochester, N. Y.

"Vitamin E in Human Nutrition," by Chr. Engel, Central Institute for Nutrition Research T. N. O., Utrecht, The Netherlands.

"Serum Vitam E Levels in Complications of Pregnancy," by Nevin S. Scrimshaw, Roy B. Greer and Ruth L. Goodland, Department of Obstetrics and Gynecology, University of Rochester, School of Medicine and Dentistry, Rochester, N. Y.

"Plasma Tocopherols in Health and Disease," by William J. Darby, Mary Ellen Cherrington, Robert H. Furman, Janet Lemley and George R. Meneely, Division of Nutrition, Departments of Medicine and Biochemistry, Vanderbilt University School of Medicine, Nashville, Tenn.

Papers Read by Title

"On the Chromatographic Separation of Tocopherols," by A. Emmerie, The Laboratory of Hygiene of the University, Utrecht, The Netherlands.

"Vitamin E in the Nutrition of Farm Animals," by D. B. Parrish, Kansas Agricultural Experiment Station, Manhattan, Kansas.

"Response of Swine to Vitamin E Deficient Rations," by F. B. Adamstone, J. L. Krider and M. F. James, Departments of Zoology and Animal Science, University of Illinois, Urbana, Ill.

"The Vitamin E Blood Level in Cows and Horses under Different Conditions," by F. C. van der Kaay, Veterinary Gynecological Institute of The Netherlands, State University, Utrecht, The Netherlands.

"The Vitamin E Content of Edible Oils Consumed in Buenos Aires (Argentina)," by Enrique Periangeli, Maria Luisa Herraiz and Herminia G. de Alvarez Herrero, National Institute of Nutrition, Buenos Aires, Argentina.

"Shute Test for Checking Unbalance Produced by Lack of Vitamin E," by J. Dutra de Oliveira, Medical School, University of Sao Paulo, Sao Paulo, Brazil.

"The Significance of Serum-Tocopherol Levels during Pregnancy," by Lauri Rauramo, Women's Clinic, University of Helsinki, Helsinki, Finland.

"Experimental and Clinical Studies on Metabolic Interrelationships of Vitamin E," by Ugo Butturini, Institute of Medical Pathology, University of Parma, Parma, Italy.

VITAMIN E IN CLINICAL MEDICINE

Afternoon Session. Chairman, R. S. Goodhart, Scientific Director, National Vitamin Foundation, Inc., New York, N. Y.

Introductory Remarks by the Session Chairman, R. S. Goodhart.

"Comparison of the Effects of Alpha-Tocopherol and Matching Placebo on Chest Pain in Patients with Heart Disease," by J. Travell, S. H. Rinzler, H. Bakst, Z. H. Benjamin and A. L. Bobb, Cardiovascular Research Unit of Beth Israel Hospital, and Department of Pharmacology, Cornell University Medical College, New York, N. Y.

"Negative Results of Tocopherol Therapy in Cardiovascular Disease," by Charles K. Donegan, Addison L. Messer, E. S. Orgain and J. Ruffin, Department of Medicine, Duke University School of Medicine, Durham, North Carolina.

"Vitamin E in Cardiovascular Disease, with Special Reference to Vascular Occlusion and Hypertension," by Evan V. Shute and Wilfred E. Shute, Department of Medicine, The Shute Institute for Clinical and Laboratory Medicine, London, Ontario, Canada.

"Tocopherol Therapy in Stasis Ulcer and Stasis Dermatitis," by Conrad Stritzler, Dermatological Services, Queens General

Hospital and Long Island College Hospital, Brooklyn, N. Y.

"Vitamin E in Rheumatic Disease," by Morris Ant and Erwin Di Cyan, Brooklyn, N. Y., and Di Cyan and Brown, New York, N. Y.

"Vitamin E and Collagen in the Rheumatic Diseases," by C. L. Steinberg, Rochester General Hospital, Rochester, N. Y.

"Tocopherols in Peyronies' Disease (A Follow Up)," by William Wallace Scott and Peter L. Scardino, The James Buchanan Brady Urological Institute, The Johns Hopkins Hospital, Baltimore, Maryland.

"Vitamin E in the Treatment of Diabetes Mellitus," by Arthur Vogelsang, London, Ontario, Canada.

Papers Read by Title

"Failure of Vitamin E to Increase Sperm Counts in Men," by Edmund J. Farris, Wistar Institute of Anatomy and Biology, Philadelphia, Penna.

"A Trial of Vitamin E Therapy in Juvenile Diabetes Mellitus," by George M. Guest, Department of Pediatrics, University of Cincinnati College of Medicine, Cincinnati, Ohio.

"Some Experiences with the Use of Vitamin E in Various Cardiac Conditions," by Samuel Baer, William I. Heine and D. Barton Gelfond, Cardiac Department, Jewish Hospital, Philadelphia, Penna.

"Tocopherol Therapy in Buerger's Diseased and Varicose Ulcers," by L. L. Pennoch, Pittsburgh, Penna.

"Alpha-Tocopherol in Alleviating Chorea," by G. C. Dowd, Boston Evening Clinic and Hospital, Boston, Mass.

"Massive Doses of Alpha-Tocopherol in Multiple Sclerosis," by G. C. Dowd, Boston Evening Clinic and Hospital, Boston, Mass.

"Tocopherols in Urethral Strictures," by Peter L. Scardino and Perry B. Hudson, The James Buchanan Brady Urological Institute, The Johns Hopkins Hospital, Baltimore, Maryland.

NEW MEMBERS

Elected April 28, 1949

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Paley, D. H., M.D., Biology. Research Assistant, Mt. Sinai Hospital, New York, N.Y.

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Alimurung, Mariano M., M.D., Internal Medicine. Research Fellow, The House of The Good Samaritan, Boston, Mass.

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Berens, Conrad, M.D., Opthalmology. Professor of Clinical Opthalmology, New York University, New York, N. Y.

Bizzozero, Orpheus, M.D., Endocrinology. Director of Education, Waterbury Hospital, Waterbury, Conn.

Broomfield, Elizabeth, B.S., Bacteriology. Bacteriologist, Control Laboratories, Burroughs Wellcome & Co., Inc.

Bruno, Leonard C., B.S., Embryology, Endocrinology. Graduate Assistant, DePaul University, Chicago, Illinois.

Clary, Mary L., A.B., Biology. Junior Pharmacologist, Ciba Pharmaceutical Products Inc., Summit, N. J.

Conrad, Lester I., B.S., Pharmaceutical Chemistry. Technical Director, American Cholesterol Products, Inc., Milltown, N. J.

Corman, Harvey H., M.D., Psychiatry. Research Fellow, Manhattan State Hospital, New York, N. Y.

Costa, Pasquale J , A.B , Pharmacology. Graduate Student, Yale University, New Haven, Conn.

Deckert, Evangeline F., M.A., Pharmacology. Director of Pharmacological Research, Reed & Carnrick, Jersey City, N. J.

Elster, Samuel K., M.D., Pathogenesis of Rheumatic Fever. Chief, Pathologic Chemistry, Army Medical Department, Research and Graduate School, AMC, Washington, D. C.

Galgan, Robert J., B.S., Botany. Graduate Assistant, De Paul University, Chicago, Illinois.

Gordon, Alvin J., M.D., Medicine. Research Assistant, Mt. Sinai Hospital, New York, N. Y.

Greenberg, Boris E., M.D., Human Fertility. Consulting Urologist, Beth Israel Hospital, Boston, Mass.

Gutmann, Helmut R., M.D., Biochemistry. Research Associate, Department of Chemistry, School of Biological Sciences, University of Tennessee, Memphis, Tenn.

Hall, Marion R., Ph.D., Clinical Psychology. Consulting Psychologist, Brooklyn Hospital, New York, N. Y.

Jeanloz, Roger W., Ph.D., Organic Chemistry & Biochemistry. Member, Worcester Foundation for Experimental Biology, Shrewsbury, Mass.

Kelly, John W., A.B., Biology. Research Assistant, University of Pennsylvania, Philadelphia, Pa.

Kiesewetter, Herbert F., B.S., Psychology. Designer, J. G. White, Inc., New York, N. Y.

King, Thomas J., M.S., Physiology, Biochemistry. Teaching Fellow in Biology, Washington Square College, New York University, New York, N. Y.

Klavano, Paul A., D.V.M., Pharmacology. Assistant Professor of Veterinary Medicine, State College of Washington, Pullman, Wash.

Kline, Paul R., M.D., Medicine. Assistant Clinical Professor, Bellevue Medical Center, New York, N. Y.

Kuta, Virginia A., B.S., Invertebrate Zoology. Graduate Assistant, DePaul University, Chicago, Illinois.

Lasagna, Louis, M.D., Internal Medicine. Resident Physician, Maimonides Hospital, Brooklyn, N. Y.

Levy, Guy W., Geology. Broker, Levy Brothers, New York, N. Y.

Loewy, Paul, M.D., Biology. Physician, Private Practice, New York, N. Y.

Loop, Anne, Ph.D., Public Health. Assistant Professor, Department of Physiology and Hygiene, Hunter College, New York, N. Y.

Lutterloh, Charles H., M.D., Biology. Practicing Physician, Hot Springs, Arkansas.

Ma, Roberta M., Ph.D., Antibiotics. Biochemist, BAIC, U.S. Department of Agriculture, Beltsville, Md.

Marelli, John A., B.S., Organic Chemistry. Student, Fordham University, New York, N. Y.

McFarren, Earl F., B.A., Biochemistry. Research Assistant, National Dairy Research Laboratories, Inc., Oakdale, L.I., N. Y.

Megibow, Samuel J., M.D., Cardiovascular Surgery. Clinical Assistant, Mt. Sinai Hospital, New York, N. Y.

Mount, David T., B.A., Virology. Assistant Instructor, Animal Disease, University of Connecticut, Storrs, Conn.

Murphy, John B., Ph.D., Biological Science. Dean, Professor of Biology, St. Louis Preparatory Seminary, St. Louis, Missouri.

Nealen, Joseph P., M.S., General and Modern Physics. Head, Physics Department, Gonzaga University, Spokane, Wash.

Nelson, Dorothy K., Ph.B., Internal Medicine. Student, Long Island College of Medicine, Brooklyn, N. Y.

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Pelcak, Emil J., B.S., Bacteriology, Chemistry. Micro-Biological Assay, Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

Polnauer, Frederick F., M.E., Bio-Mechanics, Physics. Research Engineer, New York University, New York, N. Y.

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Quinn, Robert W., M.D., Internal Medicine. Instructor in Preventive Medicine, Yale University, New Haven, Conn.

Reyes, Felix M., M.D., Biology, Chemistry. Pathologist, V. A. San Patricio Hospital, San Juan, Puerto Rico.

Robinson, Franklin, M.D., Neurology. Senior Resident, Neurology and Neurological Surgery, St. Vincent's Hospital, New York, N. Y.

Roger, Alex A., M.D., Internal Medicine. Adjunct, Cedars of Lebanon Hospital, Los Angeles, Calif.

Russ, Walter R., B.S., Pharmaceutical and Medicinal Chemistry. Research Director, Mycoloid Laboratories Inc., Little Falls, N. J.

Sachs, Bernard A., M.D., Internal Medicine. As sistant Resident in Medicine, Montefiore Hospital, New York, N. Y.

Schack, Jerome A., M.D., Biochemistry. Instructor, Army Medical Department, Research and Graduate School, Washington, D. C.

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*SECTION OF PHYSICS AND CHEMISTRY**

EXPERIMENTAL STUDIES OF ADSORPTION ON AQUEOUS SOLUTIONS

By ERIC HUTCHINSON†

Until recent years, the absorption theory of Gibbs was in good standing and acceptable to the majority of colloid chemists. This theory, which correlates the amounts of materials adsorbed at interfaces and surfaces with changes in the concentration or activity of the components of the phases whose common boundary forms the surface or interface, has been called into question a number of times of late.

In its most general form, as deduced by Gibbs,[1] the equation governing such adsorption may be written

$$-d\sigma = S^{\omega}\, dT + \sum_{1}^{r} \Gamma_i\, d\mu_i, \qquad (1)$$

where σ = surface tension,
S^{ω} = entropy per unit area of surface,
Γ_i = concentration of the i'th species per unit area, and
μ_i = chemical potential of the i'th species.

Γ_i, which is frequently, and misleadingly, referred to as a surface excess, may be envisaged in the following way. In FIGURE 1, the shaded area represents the region of discontinuity between the phases α and β. A plane, S, placed at an arbitrary level, serves to define the volumes V^{α} and V^{β} of two bulk phases. (Common sense indicates that this plane should be near, *i.e.*, within less than 100 Å, the physical boundary layer, which experiment shows to be definite to less than one or two molecular thicknesses at the most.[2]) If n_i moles of the i'th species are used in preparing the complete system, α, β, ω, and c_i^{α}, c_i^{β} are the concentrations of the i'th species in α and β, in the uniform portions of the bulk phases, then, assuming that α and β are completely uniform up to the plane S,

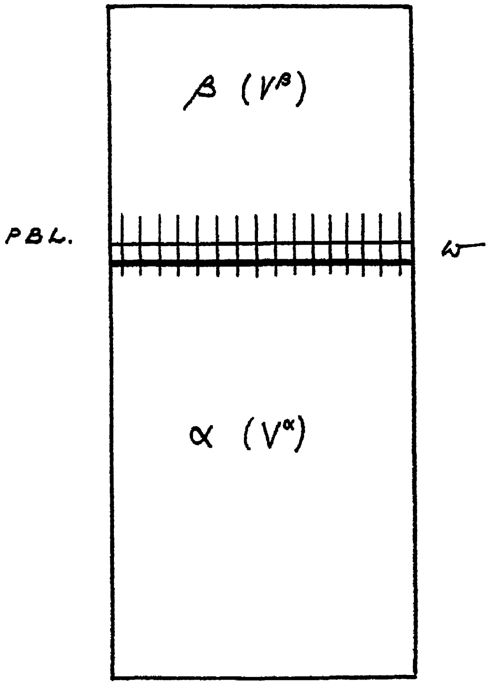

Figure 1

$$\begin{aligned} n_i &= n_i^{\alpha} + n_i^{\beta} + n_i^{\omega} \\ &= c_i^{\alpha} V^{\alpha} + c_i^{\beta} V^{\beta} + n_i^{\omega}; \end{aligned}$$

$$i.e., \qquad n_i^{\omega} = n_i - c_i^{\alpha} V^{\alpha} - c_i^{\beta} V^{\beta}. \qquad (2)$$

This equation defines the *chemical content* of the surface layer.

Now, from EQUATION 1, it may be deduced that $\sigma = \sigma(K + 1$ intensive variables), but experience indicates that the surface or interfacial tension between two phases is determined uniquely by the intensive state of the system. But the intensive state of a two phase system α and β, of K components in complete equilibrium, is determined by a number of intensive variables given by the phase rule,

---

* The Section of Geology and Mineralogy held a meeting on May 2, 1949, at which Doctor H D Thompson, Department of Geology, Hunter College, N. Y., presented a paper entitled "Structure-Drainage in the Appalachian Mountains" an abstract of which will appear in the November issue of the TRANSACTIONS This will be supplementary to his paper entitled "Drainage Evolution in the Appalachians of Pennsylvania" which won a George Frederick Kunz Prize at the Annual Meeting of the Academy in December, 1949, and which will appear shortly in the ANNALS of The New York Academy of Sciences, Volume 52, Article 2.

† Department of Chemistry, Fordham University, New York. This paper, illustrated with lantern slides, was presented at the meeting of the Section on May 3, 1949.

$$f = K + 2 - \omega,$$

where f = number of degrees of freedom

and ω = number of phases.

Hence, $f = K$ for a K component system. Therefore, it follows that

$\sigma = \sigma$ (K *independent intensive* variables)

and that, of the $K + 1$ intensive variables in the Gibbs adsorption equation, only K are independently variable.

In applying the equation to a particular problem, the experimental part consists in choosing some intensive variable x_j and measuring changes in σ caused by changes in x_j.
Then:-

$$-\frac{\delta\sigma}{\delta x_j} = S^{\omega}\frac{\delta T}{\delta x_j} + \sum_{1}^{l}\Gamma_i\frac{\delta\mu_i}{\delta x_j}. \qquad (3)$$

It can be shown[3] that a solution to EQUATION 3 is possible in practice only if temperature is chosen as one of the intensive variables; i.e., $\sigma = \sigma$ (T, $K - 1$ intensive variables, $x_2, \cdots x_l$). Then an array of equations is obtained—

$$\left.\begin{aligned} \sum_{1}^{k}\Gamma_i\frac{\delta\mu i}{\delta T} + S^{\omega} &= -\frac{\delta\sigma}{\delta T} \\ \sum_{1}^{k}\Gamma_i\frac{\delta\mu i}{\delta x_2} &= -\frac{\delta\sigma}{\delta x_2} \\ \sum_{1}^{k}\Gamma_i\frac{\delta\mu i}{\delta x_k} &= -\frac{\delta\sigma}{\delta x_k} \end{aligned}\right\}, \qquad (4)$$

in which there are $K + 1$ unknowns, viz. S^{ω}, $\Gamma_1, \Gamma_2, \cdots \Gamma_k$, for the solution of which there are only K simultaneous equations. Thus, a complete solution is possible only when a further, auxiliary equation is employed. It may be shown[3] that such an auxiliary equation can be made to define uniquely the position of the plane S, the "Gibbs surface," referred to earlier. Any equation of the type

$$\Phi(j_1, \cdots j_n, b_1 \cdots b_\theta) = 0$$

may be used to define the position of the Gibbs surface, where j_1 and $\cdots j_n$ are any set of functions of the state of $\alpha\beta\omega$, atleast one of which must be variable with the position of the Gibbs' surface, and b_1 and $\cdots b_\theta$ are any set of functions of the state of $\alpha\beta\omega$ which are not variable with the position of the Gibbs' surface and may be constants.

Any equation Φ may be used which is continuous and dimensionally correct. Thus, for example, Guggenheim and Adam[4] suggest

$$\Gamma_1 V_1^{\alpha} + \Gamma_2 V_2^{\alpha} = 0 \qquad (K = 2)$$

$$\Gamma_1 X_1^{\alpha} + \Gamma_2 X_2^{\alpha} = 0 \qquad (K = 2)$$

where V^{α} and X^{α} are, respectively, partial molal volumes and mole fractions. Gibbs himself used the equation $\Gamma_{\text{solvent}} = 0$, which, although easy to apply, is difficult to envisage in physical terms and has been accorded an altogether undeserved importance by physical chemists. The equation which is used may be designed to incorporate any physical interpretation of the nature of the adsorbed film, as, for example, the hypothesis that the adsorbed film is monomolecular.[4]

By historical accident, most experimental investigations of the adsorption equation have been restricted to two-component aqueous solutions. For such solutions, the equation takes a very simple form if the Gibbs convention $\Gamma_{\text{water}} = 0$ is used. Under these conditions, choosing as the intensive variables temperature and concentration, activity, or mole fraction, the following solutions are obtained:

$$-\frac{\delta\sigma}{\delta c_s} = \Gamma_s\frac{\delta\mu s}{\delta c_s}, \qquad (5.1)$$

$$-\frac{\delta\sigma}{\delta a_s} = \Gamma_s\frac{\delta\mu s}{\delta a_s}, \qquad (5.2)$$

and

$$-\frac{\delta\sigma}{\delta N_s} = \Gamma_s \frac{\delta\mu s}{\delta N_s}. \qquad (5.3)$$

Provided $\frac{\delta\mu s}{\delta c_s}$, $\frac{\delta\mu s}{\delta a_s}$, and $\frac{\delta\mu s}{\delta N_s}$ can be evaluated, as can usually be done, then Γ_s may be calculated.

Experiments designed to verify the Gibbs equation were reported by Donnan,[5] McBain,[6] and others,[7] and many yielded data for *directly determined* Γ's in considerable quantitative disagreement with the Gibbs' equation but in qualitative agreement in that compounds, which in aqueous solution decreased the surface tension of water $\left(\frac{\delta\sigma}{\delta c_s} < 0\right)$, were found to yield positive Γ's.

McBain and Swain,[8] in a refined experimental approach to the direct determination of Γ_s by the "microtome" method, found excellent agreement with the Gibbs equation for a number of simple solutes such as phenol and cinnamic acid. Thus, there appears to be very satisfactory agreement between the observed data and those calculated from the Gibbs equation for a number of compounds on which careful experiments have been performed.

From about 1937 onwards, however, a number of anomalous effects were observed which appeared to be in direct conflict with the Gibbs theory. A very large class of surface active materials, which are generally classified as detergents, were reported to yield, in aqueous solution, surface tension concentration curves exhibiting a minimum. These minima were found to occur at values of the surface tension between 25 ~ 40 dynes/cm. Experience clearly indicates that, with such large depressions of the surface tension of water, adsorbed material is present on the surface, and yet inspection of the Gibbs equation would appear to indicate that at the minimum, where $\frac{\delta\sigma}{\delta c_s} = 0$, Γ_s should also be zero. Hence, there seemed to be a serious discrepancy between thermodynamic and experimental fact. A number of authors[9] suggested that the Gibbs equation is incorrect and needs to be modified. This is a somewhat startling suggestion, in view of the fact that the Gibbs equation is based on classical thermodynamic concepts, not called into question in other contexts.

One highly significant factor observed with materials exhibiting minima in surface tension curves was that all such compounds become colloidal in solution above a certain critical concentration, forming micelles of the type studied by McBain and his school.[10] Moreover, the minima were observed to occur at or near the critical concentration for micelle formation.

A significant advance was made as a result of an observation by Miles and Shedlovsky[11] to the effect that, when a sample of sodium dodecyl sulfate (normally observed to give a minimum surface tension curve) is purified by exhaustive extraction, no minimum is observed. Moreover, the minimum is restored by the addition of small quantities of impurities such as dodecyl alcohol or homologs of the detergents. Extremely small amounts of alcohol, 0.05 per cent, are sufficient to cause the minimum to reappear. These observations have been extended to a number of alkyl sulfates and sulfonates by Ross and Shedlovsky.[12]

It appears highly probably that all minima may be explained in terms of small quantities of impurities. Further, the poor reproducibility of the results of independent investigators becomes understandable as a result of the extreme susceptibility of these systems to small traces of impurity.

The qualitative observations of Miles and Shedlovsky have been confirmed and demonstrated quantitatively by the author.[13, 14] It is clear that, when it contains only small traces of highly surface active impurity, a solute of the kind represented by sodium dodecyl sulfate cannot be treated, even approximately, as a single thermodynamic component. A recent theoretical investigation of the Gibbs adsorption equation as applied to multi-component systems[3] makes possible the calculation of the Γ's for both the main solute component, say sodium dodecyl sulfate, and an impurity, such as an alcohol.

Surface tension curves of a series of pure alkyl sulfates in aqueous solution, in the presence and absence of straight chain alcohols and homologous alkyl sulfates, have been determined in the author's laboratory,[14] from which Γ's for the various components have been determined.

It is found that, at concentrations of about 1 per cent of that of the detergent, a straight chain alcohol comprises about 50 ~ 80 per cent of the mixed adsorbed film in the presence of an alkyl sulfate containing the same number of carbon atoms, demonstrating the very much greater surface activity of the alcohol. The addition of the alcohol to an alkyl sulfate solution does not cause a too great change in the absolute value of Γ for the sulfate; the alcohol rather *enters* the adsorbed film than *replaces* alkyl sulfate molecules. Thus, the presence of a small amount of alcohol causes an *additional* depression of the surface tension, so that, below the 'critical' concentration, the surface tension curve of a contaminated alkyl sulfate solution is much lower than that of a solution of the pure material. Such phenomena, therefore, explain the large depressions of surface tension observed below the critical concentration with impure materials; but the problem of the minimum still remains.

Now, it is highly significant that all materials which exhibit minima form colloidal micelles above the critical concentration. The work of McBain and his school[10] shows that such micelles can "solubilize" materials, often in considerable amounts up to mole/mole ratios of 0.2 for the solubilized material and solubilizer. This process is thermodynamically reversible and may be considered in some respects as an ordinary distribution phenomenon, the solubilized molecule being distributed between free aqueous solution and the hydrocarbon-like interior of the micelle. With materials such as long-chain alcohols the equilibrium distribution is shifted strongly in favor of the micelle phase, so that the amount of alcohol in free solution is much less than that corresponding to the stoichiometric proportion of the alcohol in the whole system Hence, the activity of the alcohol in aqueous solution undergoes a rapid decrease as the concentration of the detergent is increased above the critical concentration and micelles form. Thus, the contribution of the alcohol to the adsorbed film is drastically and rapidly reduced in the region just above the critical concentration, with a consequent increase in the surface tension of the solution to values corresponding closely to those of solutions of the pure detergent. Similar arguments may be applied to any other similar surface active compounds containing impurities. Clearly, the rate of increase of surface tension beyond the minimum should depend on the surface activity of the impurity and on the relative ease of solubilization of the impurity. Experiments are being planned to verify these points.

Hence, it appears that, if proper care is exercised in the purification of materials, no discrepancies with the Gibbs adsorption theory occur for two-component systems. Further, in multi-component systems, if the number of components is known, a complete solution of the complex surface tension phenomena is possible without benefit of additional postulates beyond those originally proposed by Gibbs.

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SECTION OF BIOLOGY

THE BIOLOGICAL SIGNIFICANCE OF THE TISSUE DOSE OF IONIZING RADIATIONS

By HARALD H. ROSSI*

Although the tissue dose of ionizing radiations is, today, one of the most fundamental quantities in radiobiology, its units, as well as measurements in terms of such units, are in a rather unsettled state, except in the case of X-rays and gamma rays below a few million volts energy where determinations in terms of the roentgen are fairly well standardized. While the delay in the case of the other radiations is certainly due to the relative novelty of the problem, the matter is aggravated because of its physical complexity. Certain aspects of the problem will be presented here, with special regard to the problem of a quantitative measurement of radiation and the question as to how far the result of such a measurement serves to define the conditions under which a radiobiological experiment is performed.

Any such experiment requires some sort of dosimetry. The most primitive type, perhaps, is the one in which there is merely a question as to whether any radiation at all is reaching the object which is supposed to be irradiated. This is by no means as simple a matter as might appear at first glance. It is true that no experimenter having any knowledge of the field would expect to investigate radiobiological effects in regions where, for obvious physical reasons, there is no radiation. Thus, the beta rays of radioactive phosphorus, which penetrate only a few millimeters of tissue, would certainly not be employed as external sources for whole body irradiation of a mouse. On the other hand, they might be employed internally, and then the question as to whether any organ of the animal receives any radiation will depend on the way the isotope is administered—whether it is fed or injected, whether it is in a soluble or insoluble compound, *etc.* In other words, the physical dose will depend greatly on biochemical and physiological factors; and, under these circumstances, dosimetry, even of the vaguest kind, is by no means simple.

Dosimetry must be fairly exact, of course, when the conditions of a biological experiment are to be described so that work performed by one biologist may be repeated by another. In this case, the amount of radiation involved should be given with at least the accuracy with which the biological effect may be ascertained. In determinations of L.D. 50 (the dose required to kill half the animals of a given group), a five or ten per cent error in dose determination will usually be small compared with individual or statistical variations. However, in cases where good statistics and virtually no individual variations exist, as for instance in certain genetic work, dosimetry within a few per cent is quite desirable.

Fairly accurate dosimetry is also required when the effects of different radiations are to be compared. This brings us to the question of a unit of tissue dose, which may be used for any ionizing radiation, as contrasted with the roentgen, which is restricted to X- and gamma rays. A discussion of this question must be preceded by a brief account of the physical processes which take place when ionizing radiation interacts with matter.

---

* Radiological Research Laboratory, Columbia University, New York. This paper, illustrated with lantern slides, was presented at the meeting of the Section on May 9, 1949.

Ionization of matter is ultimately always carried out by electrically charged particles of high velocity, even though the primary radiation may consist of uncharged particles or quanta. The most important practical case in which charged particles are used directly in the irradiation of biological material is, today, probably in the use of beta ray-emitting isotopes which are used as "distributed sources," i.e., are, through injection or other means of incorporation, distributed throughout the biological material irradiated. The high-speed electrons emitted in the decay of the radioactive material penetrate from a few microns to several millimeters of tissue, depending on the kind of isotope used. If the objects to be irradiated are sufficiently small (such as unicellular organisms), the isotope may be used in external irradiation. Electrons, and even positively charged ions (protons, alpha rays *etc.*), may in some modern accelerators be made to acquire such energies that they can penetrate several inches of tissue, but these installations are thus far rarely available to the biologist. If relatively uniform external irradiation of large masses of tissue is required, use is made of primary radiations which are not ionizing *per se* and are much less strongly absorbed than ionizing particles. The interaction of these radiations with matter gives rise to secondary charged particles, which will be the more uniformly distributed in the affected tissues the less the primary "neutral" radiation is absorbed in passing through these tissues.

FIGURE 1 gives a summary of the possible reactions taking place when these radiations interact with matter. While, for the sake of completeness, all possible types of reactions are listed, only some of them are of importance when tissue is exposed to the radiation beams which are usually available.

Without going into the details of any of these processes, it might merely be pointed out that X- and gamma rays will, in all practical cases, give rise to electrons, while the only directly ionizing particles liberated by neutrons are positively charged nuclei or nuclear fragments. These differ from electrons by losing their energy much more rapidly when passing through tissue, but the mechanism of energy loss is virtually the same.

The electromagnetic fields set up by the motion of these particles cause the ejection of electrons from atoms in the matter traversed, and this leads to a trail of positive and negative ions (the latter formed by attachment of the electrons to neutral atoms) in the wake of the ionizing particle.

It is well to point out, however, that not all the energy of an ionizing particle will be spent in ionization. When an ionizing particle traverses matter, only about one half of its energy is ever spent in the production of ions. The other half is immediately expended in excitation of molecules; i.e., in displacement rather than actual ejection of electrons. Since the energy required for an excitation is generally considerably lower than the energy spent in an ionization, excited atoms will far outnumber ionized atoms. Furthermore, the excited atoms as well as the ionized atoms will radiate as they return to their normal state, and the emitted radiations will, in turn, cause further excitation. The energy of the radiations decreases steadily, and, finally, most of the energy is degraded into heat. After a short time, this heat diffuses throughout the affected material and leads in all practical cases to a negligible temperature rise. Before this ultimate dissipation, however, the heat is concentrated in very small volumes, and the temperatures (if one can speak of such on such a microscopic scale) are very considerable. These considerations form the substance of the "point heat" theory of Dessauer, a concept which is revived whenever the biological action of radiation is discussed.

While it is customary to attribute the biological effect of ionizing radiations to actual ionizations, there can be little doubt that excitations play at least some part in the process. This is suggested by experimentation with ultraviolet light, which can only initiate excitations and which often shows effects similar to the ones caused by ionizing radiations. It is believed that more excitations than ionizations are required to obtain most biological effects. However, beyond this rather vague realization, little is known. The absence of experimental information is paralleled by our inability to predict from theoretical grounds what processes should take place. In fact, even the question as to just what happens when radiation passes through pure water has not been entirely solved experimentally, and it would be quite unreasonable to expect that it will ever be

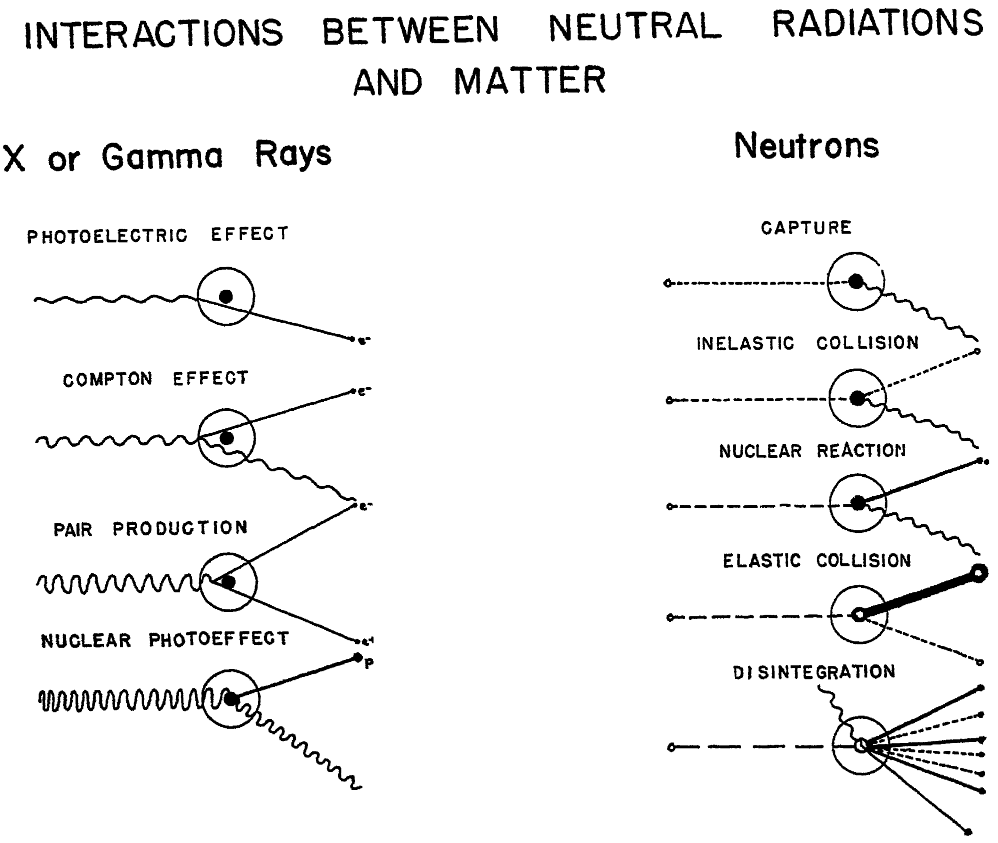

FIGURE 1. Interaction of X- or gamma rays and neutrons with matter. The atomic nucleus is represented by a solid circle and the electronic shell by a concentric open circle. The listing has been made in such a way that the energy of the primary quantum or neutron must *usually* be higher to initiate a reaction which is further down a column. The individual reactions are:

X- or Gamma Rays:

Photoelectric Effect: Absorption of a quantum and emission of an electron.

Compton Effect: The quantum is scattered with longer wave length (lower energy) by an electron which receives energy. (Scattering without change in wave length, another possible process, has been excluded, since it does not contribute to a dose.)

Pair production: Creation of an electron-positron pair near the nucleus.

Nuclear photo effect: A nuclear transformation initiated by a quantum.

The last two reactions are, at present, of no practical significance in biological experimentation.

Neutrons:

Capture: Absorption of a neutron usually followed by emission of a gamma ray

Inelastic collision: Absorption of a neutron and re-emission at lower energy. Usually accompanied by emission of a gamma ray.

Nuclear Reaction: Usually accompanied by emission of a light nuclear fragment and a gamma ray.

Elastic Collision: A fraction of the primary neutron energy is imparted to a recoiling nucleus.

Disintegration: Splitting up of a nucleus into small fragments.

The last of these processes occurs only at very high neutron energies (above about 25 MEV).

settled by a theoretical physicist. The variety and complexity of possible reactions taking place in so non-homogeneous a material as protoplasm must be enormous, and the possible number of excitations, bond breakages, bond shifts, recombinations, and back reactions must certainly be considered as running into the billions. Obviously, no physical unit of measurement can be devised which takes into account any detailed aspect of these processes.

There are only two quantities which may conceivably be used as a basis for a physical unit. One is the number of ion pairs created in the ionization process; the other is the total energy imparted per unit mass of matter irradiated. The first of these possibilities must be ruled out because, at present, we know of no way to measure ionization in a solid. We can measure ionization only in gases. However, we can deduce energy loss in a solid by measuring ionization in an enclosed gas, provided certain physical and geometrical conditions are met. The ionization in the solid cannot be accurately inferred from such measurements, although there is reason to assume that it is fairly proportionate to energy absorbed for any ionizing particle and that the absorption of one erg of energy is accompanied by about 2×10^{10} ionizations.

Energy absorbed per gram of tissue may thus be considered a satisfactory basis for a unit by both the biologist and the physicist. It seems likely that any international unit for ionizing radiation will either be based directly on ergs absorbed per gram of irradiated substance or be closely related to that quantity. At present, the most widely used (unofficial) unit of ionizing radiation is the rep (H. M. Parker), which is usually understood to be that amount of ionizing radiation which liberates 83 ergs per gram of tissue. This is roughly equal to the energy imparted to a gram of tissue by one roentgen of X-rays; a quantity which varies from about 85 to 96 ergs, depending on the wave length of the radiation. The rep is used to quantify the radiations to which the roentgen does not apply (charged particles and neutrons).

A good deal of progress has been made in the last few years in the actual measurement of tissue doses in terms of energy units. A feature of some of this work has been a tendency to make the measuring device approach the characteristics of actual tissue to a considerable extent. In fact, we have, in our laboratory, under the direction of Dr. Failla, developed a material having the same *atomic* composition as average soft tissue. This material is used in the construction of ionization chambers which have the same "response" as tissue, and the ionization observed in such chambers is a good measure of energy absorbed in tissue.

The questions arise as to just what value the results of such measurements have for the biologist and if the physicist can be of further assistance after he has supplied them.

The first point to keep in mind is that, in practice, both the roentgen and the rep are time and space integrals. That is, they give the dose received without supplying information as to when and where it was received.

The time dependence will be of importance only for those effects for which there is recovery. We know that, in many cases, a "fractionated dose" will be less effective than an equal single dose, because there may be a certain amount of recovery during the intervals between the repeated irradiations. Recovery may also occur during a protracted exposure at low irradiation levels, provided the duration of the exposure is comparable with the time required for significant recovery. On the other hand, the dose rate will be unimportant if the duration of exposure is short compared with rate of recovery. Since most sources of radiation have a rather constant output, measurement of the dose received during a certain time may be expected to yield a dose rate which is substantially constant for any time interval.

Fluctuations of the spatial distribution of energy absorption may be of several types. The most basic of these is due to the fact that the energy absorption is ultimately a statistical process. While it is customary to speak of a dose at a "point," an actual measurement can, of course, be made only with equipment of finite size, and the result of the measurement represents the average reaction of a finite amount of matter. But, even if it were possible to measure the dose received in an infinitesimal volume such as an individual atom, the outcome of the measurement would be quite trivial.

Even a dose of 100,000 r will ionize about one atom in a million, and the "dose" received by any given atom will usually be zero; and, in the rare case when it is not, it will be enormous. There is evidently no way of overcoming this difficulty in the case where it interferes with biological experimentation. Thus, in a study of mutation rate, where the dose must be limited to values which do not kill the genetic material, the probability of scoring a "hit" on any particular kind of gene will usually turn out to be rather small, and only work done with a great amount of material will have statistical significance.

Since the tissue dose is initially imparted along the tracks of ionizing particles, a given volume of tissue will be ionized twice, if at all, when its diameter is larger than the ion spacing along the track. If the biological effect under investigation requires more than one ionization in the volume, a heavily ionizing radiation will obviously be far more effective in bringing about the effect. On the other hand, if only one ionization is required, excessive ionization will lead to a "waste" of available ions, and lightly ionizing particles, such as electrons, will be more effective. Reasoning along these lines has lead to an important hypothesis in radiobiology: the "target-theory" of biological action. While it is not the purpose of this paper to analyze the details or merits of that theory, it is plain that ion density is an important characteristic of ionizing radiations. This is obvious from the well-known fact that the biological effects of equal energy doses vary considerably from lightly to heavily ionizing radiations. We have seen that X-rays give rise to sparsely ionizing electrons, while neutrons produce predominantly heavily ionizing secondaries. The fact that fast neutrons are about seven times more lethal to a mammal than X-rays shows how important the difference in ion density is. We know that the ion-spacing, as well as the spacing of excited atoms (which are relatively distributed in the same ratio), is about a hundred times larger in the tracks of electrons liberated by hard X-rays than along the paths of neutron recoils; but this figure is very rough indeed. The ion density increases along the track of any ionizing particle, and, in fact, the ion density near the end of electron tracks often may be higher than in the initial portions of a proton track. Obviously, ion density is an important factor which cannot be easily expressed in a quantitative way, and, while it is possible for instance, to say that a certain energy dose was delivered by "fast neutrons," the actual information on spatial distributions of ionization is quite vague.

Varying spatial distribution of ionization on a larger scale will result from a non-uniform deposition of radioactive isotopes in tissues. Effects of this type may be strikingly demonstrated by the use of autoradiographs in which the radiation emitted by radioactive substances, which are deposited in a tissue section, are made to affect a photographic emulsion. Here too, wide variation in distribution may be observed, and the tissue dose for a *uniformly* distributed isotope, the quantity commonly supplied by the physicist, is often of no value whatsoever.

It should be pointed out, however, that a special analysis of a given problem can be of help in many cases where a simple value of average dose is useless. A large body of statistics has been created, for instance, which may be used to evaluate biological effects of single ionizations despite the randomness of such events. Fairly good values of ion spacings, average or extreme, may be obtained by studies of radiation fields, and the problem of non-uniformly deposited isotopes may be attacked either by experimental determinations with simulated geometries or, sometimes, even by outright computation. Thus, the question as to whether radioactive phosphorus is a good agent in the treatment of leukemia because it concentrates four or five times more in the white cell than in the serum may be answered in the negative on the basis of fairly simple calculations.

Therefore, in summary, it may be emphasized that, while the biologist may, in certain cases, derive little value from conventional dosimetry, he will usually obtain significant data by collaborating on special problems with the physicist. The results of such collaboration certainly will contribute to the progress of science.

SECTION OF PSYCHOLOGY

THE NATURE OF SPACE AND VISUALIZATION ABILITIES: SOME RECENT FINDINGS BASED ON FACTOR-ANALYSIS STUDIES*

By WILLIAM B. MICHAEL†

History of Problem

Contrary to the traditional practices by which many of the older psychological concepts regarding the nature of mental abilities have been formulated, the construct of a spatial and/or visualization ability has been developed inductively through the medium of factor analysis. In a well-known investigation in which he reported the isolation and a psychologically meaningful identification of seven primary mental abilities, Thurstone[12] described a factor which resembled a group factor reported by Kelley[8] in earlier studies. This same factor appeared in later analyses reported by Thurstone[13] and by Thurstone and Thurstone.[14]

During World War II, several factorial studies carried out by members of the psychological research units of the Army Air Forces revealed that the variance associated with Thurstone's primary spatial-visualization ability could be separated into two apparently independent factors.[2] These two factors were identified as spatial-relations and visualization abilities respectively, abbreviated as S_1 and V_2. In fact, two other space factors (S_2 and S_3), which were not too readily interpretable, and a factor tentatively indentified as visual memory, appeared in several analyses. The fact that the spatial-relations ability represented one of the two most valid factors in the selection of pilots (as evidenced by its high weight in the pilot pass-fail criterion) is indicative of its practical importance.[2, 9] This factor appeared to represent primarily an ability to make accurate discriminations of the spatial relations of objects with reference to the location of the human body. The factor identified as visualization involved the process of imagining changes in positions of visual objects and required the subject mentally to twist, turn, or rotate these objects from one position to another. (An elaboration of the psychological meaning of these two factors is to follow).

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* The writer wishes to express his gratitude to the Social Science Research Council for kindly making available a grant-in-aid, without which completion of three studies to be described would not have been possible. Grateful acknowledgment is made to Drs. J. P. Guilford and Wayne S. Zimmerman for many valuable suggestions during the course of the investigations.

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The Problem

The primary purpose of this paper embraces two objectives: (1) to summarize the results of three postwar factor-analysis studies (undertaken by the writer) that were planned to test the validity of two hypotheses concerning differences in the psychological properties of the factors of spatial relations and visualization, and (2) to review the findings regarding spatial and visualization abilities in two factorial studies by two investigators[1, 16] who have reanalyzed data presented by Thurstone in his pioneer study.[13] A secondary purpose, which is an outgrowth of the first objective, is to indicate whether tests of the type employed by the AAF and Thurstone's tests have, in common, factors that can be identified to be the same. In other words, a summary is to be presented of the results of three studies which serve as a check upon the belief that the AAF factors and Thurstone's primary mental abilities are essentially identical. Special attention, of course, is to be directed toward Thurstone's spatial factor.

Investigations Planned to Test Two Hypotheses Regarding The Nature of Spatial-Relations and Visualization Factors

General plan followed. Within each of the test batteries administered, two groups of tests were incorporated which were believed to be representative of the psychological processes described in the hypothetical statements to follow concerning the nature of the spatial-relations and visualization abilities. Introspection was freely employed as a means of finding a group of tests which appeared to represent a common task. To these two groups of tests were added several reference tests of fairly well-known factorial content to assist in the identification of those portions of variance in each test of a given group associated with other intellectual factors. These reference tests, it was thought, would tend to indicate the degree of purity of each test within a group intended to measure an hypothesized function. Moreover, the incorporation of tests not planned to yield evidence regarding the tenability of the hypotheses would serve to reduce the amount of specific variance in the spatial-visualization tests considerably.

A sufficient, though not necessary, condition for the tenability of each of the hypotheses would be that a different factor is defined in the factor-analysis procedure by each of the two groups of tests. Should one or more tests within a given group contain a substantial amount of variance in the factor identified by the second group of tests or in one of the reference tests, such a finding would tend to make the evidence for the hypothesis corresponding to the first group tests less convincing but not necessarily lacking.

The samples. The two hypotheses to be tested were the same for all three samples studied. One group consisted of 360 male subjects of Rutgers University who were taking an introductory course in psychology. Of the group, 220 were freshmen and sophomores, and 140 were juniors and seniors majoring in nearly every department of the University. Slightly more than half were veterans. The age of the subjects varied from 16 to 34. The median age was 22 years. Two high school samples, one consisting of 151 boys and the other of 139 girls, in the twelfth grade of John Muir Junior College in Pasadena, California, were also employed. The age of these subjects ranged for each sex group for 15 to 20, the median age being 17 years in each sample.

The tests. To the college group, fourteen tests were administered: six factor tests of the Guilford-Zimmerman *Aptitude Survey* (*Verbal Comprehension*, *General Reasoning*, *Numerical Operations*, *Perceptual Speed*, *Spatial Orientation*, and *Spatial Visualization*) and eight tests once employed by Thurstone* in his experimental studies (*Completion*, *Number Series*, *Identical Forms*, *Cubes*, *Flags*, *Punched Holes*, *Pattern Ana-*

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* The writer wishes to express his appreciation to Professor Thurstone for his permission to reproduce several of his tests for inclusion in the batteries employed in these three studies.

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logies, and *Form Board*). The six tests of the Guilford-Zimmerman *Aptitude Survey* were of a type similar to those used in AAF, although they represented a completely new set of tests with many improvements.[6,7] To the two high school samples, the same six tests of the Guilford-Zimmerman *Aptitude Survey* were given. Four of Thurstone's experimental tests, *Cubes*, *Form Board*, *Punched Holes*, and *Identical Forms* were used along with four tests from the Thurstone's battery of *Primary Mental Abilities* (long forms): *Number*, *Verbal*, *Space*, and *Reasoning*. A part of a test by Wrightstone titled *Spatial Relations* was also used, although the content of the test items suggests that it probably has a larger proportion of its variance in visualization than in spatial-relations.

For the college sample, the two groups of tests used to check upon the validity of the two hypotheses regarding the nature of spatial-relations and visualization factors consisted respectively of (1) *Cubes*, *Flags*, and *Spatial Orientation*, and (2) *Form Board*, *Punched Holes*, and *Spatial Visualization*. For the two high school samples, the group of tests intended to furnish evidence regarding the tenability of the first hypothesis as to the nature of spatial-relations factor consisted of *Cubes*, *Spatial Orientation*, and *Space* (by the Thurstones), and the group of tests chosen in conjunction with the factor of visualization included *Form Board*, *Punched Holes*, *Spatial Visualization*, and perhaps *Spatial Relations* (by Wrightstone).

Hypotheses. In a recent article[10] in which the findings for the college sample were reported, the two hypotheses regarding the nature of the spatial-relations and visualization factors were formulated in considerable detail. Accompanying the statement of each hypothesis was a description of the manner in which each test in the group of tests selected was thought to conform to the type of task relevant to the corresponding hypothesis. Since a clear understanding of the hypotheses is essential for subsequent interpretation of the results, the recent formulations are quoted in their entirety:

> The factor of spatial relations was hypothesized to represent the ability to comprehend the ***arrangement*** of elements within a visual stimulus pattern primarily with reference to the human body. Thus, an important implication in the ability to perceive spatial arrangements is that the subject is able to distinguish whether one object is higher or lower, left or right, or nearer or farther than another within the same field. Through the presentation of two simulated views of a stimulus pattern, a test item may be constructed such that there is a systematic relationship between the order of elements within the first spatial pattern (the stimulus component of a test item) and the order of elements within the second pattern (the response component of a test item).
>
> For example, in Thurstone's *Cubes* test the examinee is asked to recognize whether the designs on the sides of a second cube can hold the same relationship to one another as they do on the first cube. By noticing within each cube the left-right, top-bottom, and front-back interrelationships of the faces, the subject is able in each item to refer the locations of three designs on three exposed faces of one cube to the locations of designs on the faces of the other cube. In Thurstone's ***Flags*** test the examinee is required to tell whether the exposed faces of the American flags of identical size can represent the same side of the flag. Relating corresponding left-right and top-bottom boundaries (outlines) of the two flags appears to be an important aspect of the solution. Similarly, in Guilford and Zimmerman's test of ***Spatial Orientation*** a premium is placed upon the examinee's maintaining the correct relationship of objects to one another in background scenery that has been viewed twice from a motorboat—first before and then after its prow has moved up or down and/or left or right In the test the examinee is asked to determine the relative amount and direction of movement of the boat corresponding to changes in the two views of the background setting.
>
> The factor of visualization was hypothesized to represent an ability that requires the mental ***manipulation*** of visual images. In contrast to another factor identified as visual memory (2), which appears to be a static or reproductive form of visualization, the factor referred to as visual manipulation, or simply visualization, is dynamic. This visual manipulative ability appears to be present in the solution of problems in which the individual finds it necessary mentally to move, rotate, turn, twist, or invert one or more objects. Following the performance of the presented manipulation the individual is required to recognize the new position, location, or changed appearance of the object or objects.
>
> Three tests selected to yield evidence for the second hypothesis included two by Thurstone, ***Punched Holes*** and ***Form Board***, and one by Guilford and Zimmerman, ***Spatial Visualization***. In the test of ***Punched Holes*** the examinee is presented a symbolic representation of a folded sheet of paper into which one or more holes have been punched and is required to imagine where the holes will be when the sheet is unfolded. In the second Thurstone test the

> examinee apparently finds it necessary in each item mentally to turn, rotate, or invert two or more flat geometric figures in such a way that they can be placed together to fit within the outline of a larger geometric figure. In each of the tests, the examinee is asked to record the final positions respectively of the holes and of the geometric figures. In the test of *Spatial Visualization* the subject is required mentally to turn, tilt, or rotate a three dimensional object—an alarm clock—drawn on a sheet of paper into a final position according to written instructions. As alternative responses the pictures of the clock are presented in five positions, one of which is correct.
>
> Whereas in the two Thurstone tests the examinee is required to draw in his solution to the problem, in the third test he merely selects as his solution one of five choices presented. It is quite likely that in addition to measuring visual manipulative ability other factors are involved in the three tests—factors reflecting the manner in which responses to the items are recorded.
>
> Another important difference in the nature of the psychological processes hypothesized for the spatial-relations and visualization factors was that of speed of response. As indicated by findings in the AAF Aviation Psychology Program, the tests thought to measure the spatial-relations factor were administered with fairly short time limits, but those tests thought to measure visualization were given with fairly liberal time allowances. The spatial relations factor was considered to demand a fairly rapid decision on the part of the examinee as to the spatial position of objects with reference to his own location; whereas, the visualization factor was believed to be represented in problems requiring a more deliberate and less automatic approach. In part such a distinction may be a function of the complexity of a task (i.e., the number of steps entering into the performance of an item), the more complex tasks requiring visualization for their solution.[10]

(The only additional point which should be made in the instance of the two high school samples concerns the test *Space* by the Thurstones. This test, consisting of three exercises, or sub-tests: *Flags, Figures,* and *Cards*, should be expected to be loaded primarily on the spatial-relations factor, inasmuch as the items appear to represent essentially the same type of task found in Thurstone's experimental test *Flags.)*

Factor analyses. The Thurstone centroid method of factoring was employed for the three matrices of test intercorrelations. In order that a psychologically meaningful interpretation of the factors might be realized, the axes were rotated by pairs to positions satisfying Thurstone's criteria of simple structure and positive manifold. A graphical method devised by Zimmerman[15] was employed in rotation.[10] In the three analyses, six rotated factors were meaningfully identified as verbal comprehension (*V*), general reasoning (*R*), numerical facility (*N*), perceptual speed (*P*), spatial-relations (*S*), and visualization (V_z). In each analysis, two other factors appeared that could not be readily interpreted, although work upon the two analyses for two high school samples is still in progress, with the view that certain minor adjustments may bring about a plausible interpretation for at least one more factor. With the exception of the spatial-relations and visualization factors, the other four factors *V*, *R*, *N*, and *P* appeared to be identical with those previously found by Thurstone.

Evidence regarding the validity of the two hypotheses. The hypotheses regarding the nature of spatial-relations and visualization abilities were upheld, since, in the main, variances associated with these two factors were limited to the respective groups of tests initially inserted within the test batteries to bring out the factors. However, it should be pointed out that in the three analyses some of the tests in these groups tended to be complex factorially —that is, they were weighted in both the spatial-relations and visualization factors. Nevertheless, the larger component of variance in a complex test was in the factor common to the group in which that test appeared.

Introspective reports obtained from several of the subjects revealed that, for a given test, some individuals depended upon one psychological process— others, upon another. For example, in the *Cubes* test, many a subject reported that he matched up the locations of the sides of the cubes with respect to his own position—left face in the second cube with what was front face or back face or right face in second cube, top face with what was front face or right face or left face. Such a process would be considered to represent a spatial-relations ability. On the other hand, if an individual reported that he picked up the first cube as if it were a block or die and proceeded to rotate it into a final position which matched (or could not match) the second cube, then the process of visualization would be dominant. In their introspections, some individuals revealed that they put to use both processes.

Inasmuch as the results based on data obtained from the high school samples studied are still somewhat tentative at the time of the writing of this paper, detailed information regarding the magnitude of the factor loadings will not be presented. Suffice it to say, the two factors of spatial-relations and visualization were readily identified. The two hypotheses were upheld. The pattern of loadings in the two groups of tests resembles closely that found in the two similiar groups of tests employed in the college sample.

The factor loadings to be reported will be confined to those in the six key tests (two groups of three tests) which defined the two space factors for the college sample. In the first group of three tests, *Spatial Orientation*, *Flags*, and *Cubes*, which were incorporated to bring out a possible spatial-relations ability, the loadings were respectively .58, .44, and .43 in the factor identified as spatial-relations. In the visualization factor, the weights in these three tests were, respectively, .42, .14, and .20. Loadings in the visualization factor of .62, .52, and .52, respectively, were obtained in the three tests of the other group: *Spatial Visualization*, *Punched Holes*, and *Form Board*. For the spatial-relations factor, the weights were .44, .25, and .21, respectively. A loading of .30 or larger is probably indicative of a significant amount of variance in a given factor in view of the size of the sample (N = 360).

Reanalysis of Thurstone's Data

Fruchter's analysis. In his investigation of the nature of verbal fluency, Fruchter[1] reanalyzed a sub-matrix of the intercorrelations of twenty tests specially chosen from the battery of fifty-seven variables used by Thurstone in his pioneer study at Chicago.[12] He found two independent factors which were identified as spatial-relations and visualization. His results agree rather closely with those obtained from the college study (Rutgers) with respect to relative magnitudes of factor loadings in the definitive tests. The loadings in spatial-relations and visualization factors, respectively, for the four tests, *Cubes*, *Flags*, *Form Board*, and *Punched Holes* were .77 and .07, .72 and .29, .50 and 6.2, and .34 and .63.*

Zimmerman's analysis. Recently, Zimmerman[16] has rerotated the thirteen centroid axes for all fifty-seven variables and has defined thirteen psychologically meaningful (orthogonal) factors. Two of these factors were readily interpretable as representing independent spatial-relations and visualization abilities. It is interesting to note the comparability of the loadings in these two factors with those of Fruchter's for the same tests. Loadings in the four tests, *Cubes*, *Flags*, *Form Board*, and *Punched Holes* were for the spatial-relations and visualization factors respectively, .59 and .30, .73 and .37, .32 and .62, and .27 and .62. These sets of loadings may be compared with those obtained by Thurstone in the one spatial factor: .62, .64, .42, and .34. Other tests among Thurstone's fifty-seven which were substantially and primarily weighted in spatial-relations factor upon reanalysis were *Block Counting*, *Lozenges B*, *Surface Development*, *Pursuit*, and *Hands* with loadings, respectively, of .52, .60, .50, .51, and .55. A test which appeared to require a considerable amount of visual manipulation of figures was *Lozenges A*, with a weight of .54 in visualization.

Not too much confidence should be placed in direct comparisons between Zimmerman's and Thurstone's findings with respect to the two space factors, since there was a noteworthy redistribution of variances for some tests on several other factors. Nevertheless, with regard to the four tests, *Cubes*, *Flags*, *Form Board*, and *Punched Holes*, which were not heavily loaded in any other intellectual factor either in Thurstone's analysis or in Zimmerman's reanalysis, it is noteworthy that the sums of squares of loadings for the factors of spatial-relations and visualization in each test, respectively, closely approximate the square of the factor loading in the spatial factor found by Thurstone. In short, the variance in Thurstone's spatial factor appears to have been resolved into two independent components.

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* That the magnitudes of these factor loadings are larger than those in the college study cited is due to the fact that shortened forms of Thurstone's tests were used It is possible to estimate a change in the magnitude of factor loadings with change in length of a test if the tests are homogeneously lengthened.[3]

Recommendations

There is much need for other empirical studies to test the validity of the two hypotheses. It is urged that similar batteries of tests be administered to other homogeneous samples in which such variables as age, occupational classification, amount and type of formal education, and sex membership be systematically varied. Particular attention should be directed toward ascertaining whether the spatial-relations and visualization abilities are correlated. In the three empirical studies undertaken by the writer, the amount of correlation has appeared to be negligible, although the number of tests employed is probably not sufficient to demonstrate, in a dependable manner, the presence of correlation among the abilities. It is also desirable that other hypotheses be formulated and submitted to verification through use of tests devised to reflect the operations implied by the hypotheses. These tests should be combined with reference tests of well-known factorial content, in order that other possible sources of variance in the experimental tests may be defined.

Once the spatial domain has been adequately mapped, greater emphasis can be placed upon a program of test development in which relatively pure tests can be constructed to measure the abilities identified. Even if some of the tests turn out to be factorially complex, means are available for attaining estimates of pure factor scores through use of suppression tests.[4]

Along with tests of known factorial content, numerous measures of criterion activities may be analyzed with the view of understanding better the factorial composition of these measures. Steps may then be taken to use tests which overlap to a minimum degree in the prediction of complex criteria. Such a procedure should lead, on a long range basis, to an economical program of test development.[3]

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Note The Section of Anthropology held a meeting on May 23, 1949, at which Doctor H. L. Shapiro, Chairman and Curator of Physical Anthropology, The American Museum of Natural History, New York, N. Y., presented a paper entitled "The Maori in Modern New Zealand," illustrated with lantern slides This paper will be published in a later issue of the TRANSACTIONS

DIVISION OF MYCOLOGY[*]

INFLUENCE OF NUTRITION ON GROWTH AND MORPHOLOGY OF THE DERMATOPHYTES[†]

By LUCILLE K. GEORG[§]

Nutritional studies of the dermatophytes have been undertaken for the purpose of determining the optimal requirements for growth and spore production in these fungi. Such a study is important in order to improve both isolation and identification techniques and, thus, increase the certainty of diagnosis in ringworm disease.

Many classifications have been proposed for the dermatophytes, the first being that of Sabouraud, which was based primarily on the clinical picture or type of lesion. Others took into consideration more botanical characteristics. In 1934, Emmons,[1] in an attempt to simplify the study of these forms, proposed a classification which is based largely on the type of macroconidium, or large septate spore, characteristic of these fungi. The production of spores has, thus, become very important in the identification of the dermatophytes.

Difco, Sabouraud's dextrose or maltose agars, now used extensively for isolation purposes as well as for the production of characteristic colonies, have not been found completely satisfactory for the study of microscopic characteristics, and a variety of media such as corn meal or potato dextrose agar have come into use. However, for those forms which produce macroconidia sparsely, such as *Microsporum audouini* and many of the trichophyton species, these media are inadequate, and new media are constantly sought in order to induce or increase spore production in these strains.

A great stimulus was given to the study by the introduction of the "natural media of polysaccharide base" by Langeron and Milochevitch[2] in 1930. On these media which consisted of whole grains of wheat, barley, corn, and oats, they were able to induce the formation of spirals, microconidia, and macroconidia in many strains of trichophytons which had not previously shown these structures on the usual Sabouraud's media.

The stimulation of growth and spore production on natural media suggested that these substances are rich in growth factors, vitamins, and amino acids which may be required by these organisms for their normal development. Recent studies, using synthetic, chemically-defined media, by Robbins *et al.*,[3] Schopfer and Blumer,[4] Burkeholder and Moyer,[5] Mackinnon and Artagaveytia-Allende,[6] Georg,[7] and others have shown, in fact, that many of these fungi actually have deficiencies for certain of the vitamins and amino acids and are stimulated by other growth factors present in natural products such as peptones, liver, and yeast extracts.

Robbins and Ma,[8] in their study of "Growth Factors for ***Trichophyton menta-***

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[*] Doctor Samuel M. Peck has requested the Editor of TRANSACTIONS to publish a corrected title for his paper, read before the Division of Mycology of the Academy on January 28, 1949, and presented by him for publication under the title: "A New Method for the Evaluation of Antifungicidal Chemicals." The author desires to correct this title to read: "A New Method for the Evaluation of Antifungal Chemicals"

[†] This study was accomplished through a grant-in-aid from the U. S. Public Health Service.

[§] Department of Dermatology, College of Physicians and Surgeons, Columbia University, New York, N. Y. This paper, illustrated with lantern slides, was presented at the meeting of the Division on May 27, 1949 *grophytes*," have shown that the normal sporulating form of this organism requires at least one amino acid for growth. Inorganic nitrogen in the form of NH_4NO_3 is almost completely unavailable to this fungus. However, the culture grows more rapidly with peptone, casein hydrolysate, and other natural products than with asparagine as the only source of nitrogen. No evidence was obtained for the requirement of any particular amino acid, any one of 14 tested being able to act as a source of nitrogen, and the fungus was autotrophic as far as the vitamins were concerned. These authors further demonstrated that the mutant or pleomorphic forms of *T. mentagrophytes* (those forms which produce rapidly growing, white, fluffy colonies composed largely of sterile mycelia) were physiologically different from the normal sporulating form. Not only can the pleomorphic form utilize asparagine much more readily, but it has the ability to use inorganic nitrogen such as ammonia.

Conant,[9] in his study of the microsporums, discovered that species of animal origin (*M. canis and M. gypseum*) grew profusely on polished rice grains and produced macroconidia in abundance, but that *Microsporum audouini*, on the other hand, grew poorly, showing no aerial growth and producing no macroconidia. This suggested that *M. audouini* was a deficient organism. Other findings led to the same conclusion. Benedek[10] found accidentally that a culture of *M. audouini*, contaminated with a bacillus which he named *Bacillus weidmaniensis*, was able to grow well on polished rice grains and produced macroconidia. Hazen[11] demonstrated that yeast extract added to honey agar resulted in a marked increase in the vegetative growth of *M. audouini* and in the production of macroconidia. The author suggested that growth factors essential to the development of *M. audouini*, present in yeast extract and produced by *B. weidmaniensis*, are probably identical. Hazen recommends a honey-peptone agar with 5 mg. yeast extract per ml. of medium for the production of macroconidia in *M. audouini*.

Benham[12] has studied the effect of nutrition on the development of macroconidia in *Trichophyton rubrum*. Here, as with the other dermatophytes, the production of the characteristic macroconidia is of great importance in the identification of the fungus. The macroconidium of *T. rubrum* are characterized as long, narrow spores of 3 to 10 septa, with nearly straight, parallel sides and blunt or pointed ends. These from the most distinctive feature of this species. They are, however, inconsistently produced on the usual media. By the use of a heart-infusion-tryptose agar, Blood Agar Base (Difco), Benham was able to induce the production of macroconidia in 49 of 50 strains studied on this medium. Blood Agar Base differs from Sabouraud's medium in reaction, having a pH of 6.8, in the absence of sugar, and in containing beef heart infusion and "tryptose," a meat digest with a higher percentage of proteose than the usual peptones. Tryptose appears to be the ingredient responsible for the production of macroconidia of *T. rubrum* on this medium.

The group of so-called "faviform trichophytons" has been of particular interest in the study of the relationship of nutrition to growth and production of spores. The organisms of this group, in which we include *Trichophyton schoenleini*, *Trichophyton faviforme*, and *Trichophyton violaceum*, have been characterized in the past largely by their deficiencies. On the usual Sabouraud's dextrose agar, they produce slow-growing, usually glabrous colonies consisting of irregular mycelia and many chlamydospores. However, by the use of whole grain media, Langeron and Milochevitch[2] have demonstrated that a rapid downy to fluffy growth with microconidia may be obtained with some of these strains.

Trichophyton schoenleini, the causative agent of the disease favus, is the most rapidly growing member of this group. On Sabouraud's dextrose agar, the average strain will produce a colony of 30 to 50 millimeters in diameter by six weeks. The growth is frequently heaped in the center and may show heavy folding. Some strains stay moist and glabrous, but most develop a small amount of white powder or short, white down over the surface of the colony. The mycelium is extremely irregular, showing the characteristic "chandeliers" and many chlamydospores.

In a study of six strains of *Trichophyton schoenleini*, using chemically-defined, vitamin-free media, we found that all of the strains grew well on basal dextrose agars with either NH_4NO_3 or acid digest, vitamin-free casein as source of nitrogen. The growth was not stimulated by any of a large series of vitamins tested. Some increase in growth and production of an aerial mycelium was, however, observed by the addition of heart-infusion or rich peptones such as Bacto-Tryptose. Culture on whole grains also improves the growth and a few microconidia were obtained on such media. These natural substances could not be substituted for by large amounts of any of the vitamins tested or by casein hydrolysate. Apparently, this is not a vitamin- or amino acid-deficient organism, although it does require some factors present in natural products for the production of spores.

Trichophyton faviforme, under which we include the album, discoides, and ochraceum species of Sabouraud, has, on the other hand, been shown to have complete deficiences for certain vitamins. This organism, which produces a large-spored, ectothrix invasion of the hair, is a common agent of ringworm in cattle and horses which is frequently transmitted to man. These organisms grow extremely slowly and poorly on the usual Sabouraud's dextrose agar, often producing colonies of not more than 10 millimeters in diameter after six weeks. Microscopically like *Trichophyton schoenleini*, the only structures seen are highly irregular mycelia and large numbers of chlamydospores.

Studies on synthetic, vitamin-free media by Robbins, Mackinnon, and Ma[3] with one of these large-spored, ectothrix organisms of animal origin, *Trichophyton discoides*, indicated that this fungus required three vitamins for growth: i-inositol, thiamine, and pyridoxine. Mackinnon and Artagaveytia-Allende[6] studied a series of 12 strains and found them to require only two vitamins, inositol and thiamine.

In a series of 28 cases of suppurative ringworm contracted from cattle, caused by *Trichophyton faviforme* and described by Fowle and Georg, [13] it was noted that the usual Sabouraud's dextrose agar was inadequate for isolation of the causative fungi in many of these cases. These authors recommended a heart-infusion-tryptose agar such as Blood Agar Base (Difco), to which has been added 0.1 mg. thiamine per 100 ml. Antibiotics such as pencillin and streptomycin may be added to inhibit bacterial growth. On such media, *Trichophyton faviforme* may be readily isolated from most cases. The colonies develop rapidly, with a downy to fluffy surface growth with large numbers of microconidia and occasionally a few macroconidia. Studies[7] with single spore isolates indicated that the three colony types described by Sabouraud as *T. album*, *discoides*, and *ochraceum* could be obtained from a single spore. It was recommended that *T. faviforme* be given as the type species of this group.

Nutritional studies by Georg[7] on synthetic, vitamin-free media with a group of sixteen recently isolated strains, as well as four stock cultures from the Culture Bureau in Holland, gave the following results: Eighteen of twenty strains of *Trichophyton faviforme* showed complete vitamin deficiences for inositol and molecular thiamine. Inositol produced some slight effect alone, but allowed only a very limited amount of growth. The minimal effective concentration was 5 gamma per 5 ml. of NH_4NO_3 broth. Thiamine showed no action alone, but, in the presence of an adequate supply of inositol, the amount of growth was proportional to the amount of thiamine present. The minimal effective concentration of thiamine was 0.01 gamma per 5 ml. of NH_4NO_3

broth. On synthetic, vitamin-free agar, with either NH_4NO_3 or acid-digest casein as nitrogen source and adequate supplies of the required vitamins, *T. faviforme* will produce rapidly growing colonies with downy to fluffy surface growth with large numbers of microconidia and, in most instances, macroconidia.

Recently, we have made a study of a third member of this group, *Trichophyton violaceum*, a common cause of chronic dermatophytosis and scalp ringworm in northern Africa, Spain, Russia, and China. The disease occasionally appears in the United States, and the characteristic endothrix invasion of the hair is well known. This fungus grows very slowly on Sabouraud's dextrose agar, producing small, moist, glabrous, usually highly verrucose colonies with a violet to deep purple pigment. Microscopically it is composed of a poorly-formed, irregular, twisted mycelium and many chlamydospores. The cultures tend to die unless frequently transferred to fresh media, and, on subculture, undergo changes of two types: (1) pleomorphism, or development of rapidly growing areas of white, woolly, sterile mycelia which may eventually cover the entire colony; and (2) development of lightly pigmented or white sectors of flat, spreading growth covered with a short, fine down which may occasionally bear a few very delicate microconidia.

There is some evidence in the literature to indicate that *Trichophyton violaceum* is a "deficient fungus" and requires special media for good growth and the production of spores. Burkeholder and Moyer[5] have shown that a strain of *T. violaceum* which they studied was deficient for thiamine and that its growth was stimulated by additions of liver and peptones.

We have recently studied a series of eleven strains of *Trichophyton violaceum*. These cultures showed all variations in colony form, and ten of the eleven cultures showed characteristic microscopic morphology on Sabouraud's dextrose agar. The more recently isolated, moist, glabrous colonies were composed largely of chlamydospores and fragments of irregular hyphae. The older stock cultures, most of which had lost the purple pigment and showed some downiness, had a very fine mycelium which showed clubbed ends and balloon-like chlamydospores. Some of the colonies showed spiny surface growth which consisted of ropes of twisted mycelia resembling coremia. By a diligent search in some of these cultures, mycelia could be found which bore a few delicate microconidia.

In contrast to these ten typical-appearing strains, a white fluffy strain, obtained from the Culture Bureau for Fungi, Delft, Holland, gave the appearance of a completely pleomorphic culture. However, on examination it was found that the aerial mycelium was covered with many microconidia. This culture, #365, proved to be completely different in its nutritional requirements from the other ten strains studied, and perhaps this accounted for the difference in its morphological characteristics.

The ten typical strains could be maintained on a vitamin-free dextrose agar with either inorganic or organic nitrogen. Availability of nitrogen was in the following order: acid-digest casein $> NH_4NO_3 > NH_4Cl > NaNO_3$. Development on such media, however, was very restricted and consisted of a thin submerged growth. Increasing the amount of available nitrogen did not increase the amount of this growth.

A survey of the vitamin requirements of these strains indicated that the growth of the ten similar strains was greatly increased by the addition of thiamine to either the NH_4NO_3 or the acid-digest casein agar. The only other vitamin which had a stimulatory effect was para-amino-benzoic acid. The effect was slight and not apparent unless 300 to 500 gamma were added to 5 ml. NH_4NO_3 broth. (That this effect was not due to contamination with thiamine was determined by testing the P.A.B. solution with washed spores of *Phycomyces blakesleeanus*.) The stimulatory effect of thiamine ranged from 0.002 gamma to 0.5 gamma in 5 ml. NH_4NO_3 broth. Further additions caused no increased growth, but even very large doses had no inhibitory effect. Only the pyrimidine portion of the molecule was active. Molecular thiamine was replaced by an equi-molar amount of 2 - methyl - 5 - ethoxymethyl - 6 - amino - pyrimidine. Oxalacetate was found to substitute partially in a manner similar to that observed by Benham[14] for *Pityrosporum ovale,* an organism which is also stimulated by thiamine.

The addition of maximal effective dosages of thiamine to NH_4NO_3 or acid-digest casein agars allowed rapid growth of these strains with downy to fluffy aerial mycelia with numerous microconidia. Although the strains grew well on thiamine-enriched NH_4NO_3 agar, a more rapid and heavier growth was obtained on thiamine-enriched casein agar. On a 0.5 per cent casein medium with 50 gamma thiamine per ml., heavy folded colonies were obtained with considerable surface down. Not only were microconidia produced in large numbers, occurring along the mycelium (*en thyrse*), and terminally in large clusters (*en grappe*), but the macroconidia of this species were observed for the first time. The macroconidia observed were 2, to, 5 celled, smooth-walled, hyaline, elongate structures borne both terminally and laterally on the mycelium. The walls are thin and frequently indented at the septae. The large majority are slightly clavate and resemble rather closely the macroconidia of *T. mentagrophytes*.

Strain #365 showed no growth on NH_4NO_3 basal agar even when large amounts of thiamine or any combination of the vitamins were added. It grew well, however, on vitamin-free casein agar. It was apparent that the deficiency here was for some amino acid or combination of amino acids. On a 0.5 per cent casein agar, a rapidly growing, heavily folded colony was obtained with a fluffy aerial growth containing large numbers of microconidia and also macroconidia similar to those produced by the thiamine-deficient strains. By testing amino acids in groups and singly, it was found that growth on the NH_4NO_3 agar could take place only when the amino acid histidine was added. The effective concentration ranged from 0.0003 mg. (300 gamma) to 0.08 mg. in 5 ml. NH_4NO_3 broth. With doses over 0.15 mg., toxic effects were observed. Histidine, alone, only allowed a meagre growth on NH_4NO_3 agar, which indicated that the addition of other amino acids would be necessary to produce growth comparable to that observed on the casein agar. The remaining amino acids were added singly and in combinations with histidine. The most available fall in the following order: glutamic > aspartic > arginine > lysine. Hydroxyproline was found to have a partial inhibitory effect in a dilution as high as M/1000. Complete inhibition occurred with 3 mg. hydroxyproline in 5 ml. NH_4NO_3 broth containing 10 gamma of histidine. This inhibition could be overcome by adding proline to the medium. Robbins and McVeigh[15] have reported a similar inhibition of the growth of *Trichophyton mentagrophytes* by hydroxyproline which could be overcome by proline. It seems apparent that these hydroxyproline-sensitive strains require proline in their metabolism.

In the presence of thiamine, the ten thiamine-deficient strains showed stimulation by amino acids. Aspartic acid was the most readily available. Other amino acids with stimulatory effects in a dilution of M/1000 were arginine, glutamic, histidine, phenylalanine, and proline. No inhibitory effect due to hydroxyproline was observed when these strains were grown in NH_4NO_3 broth. In casein broth, however, slight inhibitory effect was observed with 15 mg. hydroxyproline. This corresponds to the findings of Robbins and McVeigh,[15] who showed that the normal form of *Trichophyton mentagrophytes* (the amino acid-requiring form) was much more sensitive to hydroxyproline than the pleomorphic form which can utilize inorganic nitrogen.

The fact that one of the eleven strains

studied was physiologically different from the rest suggested that this might be an unusual mutant form or even a different species. However, by inoculating a spore suspension of this culture into the shaven side of a rabbit, an infection with erythema, scaling, and endothrix invasion of the hairs was observed. The culture recovered was identical to the culture inoculated, except that a purple pigment was visible on the reverse side of the colony. The type of infection and the recovery of the fungus with purple pigment indicated to us that this was indeed *Trichophyton violaceum.* This was further substantiated by that fact that at one time, daughter colonies found scattered around a colony of the white, fluffy culture appeared to be of the more usual purple, glabrous type. These were subcultured and a pure glabrous dark purple form was obtained. This culture, #365a, was apparently a back mutation to the common form. Upon study, it was found not only to have lost its deficiency for histidine, but to have acquired a deficiency for thiamine. This strain proved to be identical to the ten other thiamine-deficient strains studied.

Summary

(1) The production of characteristic spores—particularly the macroconidium—is necessary in the identification of the dermatophytes.

(2) It has been shown that the presence of certain vitamins, amino acids, and other growth factors present in natural products are necessary for good growth and the production of spores in many of the dermatophyte species. This is true for *Microsporum audouini*, *Trichophyton rubrum*, *Trichophyton schoenleini*, *Trichophyton faviforme*, and *Trichophyton violaceum.*

(3) The macroconidia of *Trichophyton violaceum* have been described for the first time.

(4) It is suggested that for certain forms, which show complete or almost complete deficiencies, special enriched media be used for isolation and maintenance of these cultures.

(5) Further studies of this type may be of value in the promotion of good growth and the production of spores in other dermatophyte species.

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SECTION OF GEOLOGY AND MINERALOGY*

DISTRIBUTION OF PEBBLES IN A GLACIAL OUTWASH PLAIN

By A. C. HAWKINS†

The object of this paper is to give quantitative factual evidence as to the distribution of pebbles of various rock types in a deltaic sand deposit, with respect to changes in size and shape resulting from a combination of the effects of transportation by running water in the presence of moving sand and the erosion effects due to natural differences in mineral composition.

The following data have been collected by the writer in a typical area of the outwash plain of the Wisconsin glacial moraine in east-central New Jersey. The pebbles have been classified as to petrography and geologic age as closely as possible. They have also been classified as to size and roundness. Degree of roundness has been determined by inscribing the largest possible circle within the pebble's outline and comparing this circle's area with the sum of the areas of three circles inscribed in three of its subangular or rounded corners.‡ Perfection of rounding would thus be represented by the ratio 1:3, which none of the pebbles have attained. No complete conclusion has been made or attempted about the relative effects of distribution by water-flow as compared with the effects of abrasion and weathering.

TABLE 1

PLEISTOCENE GEOLOGY IN UNION AND SOMERSET COUNTIES, NEW JERSEY

Distribution of Pebbles in a Portion of the Glacial Outwash Plain (Wisconsin), Plainfield to Bound Brook, N. J.

	Variation in size (averages in cm) Distance s. of terminal moraine			*Variation in roundness (averages)*
Rock type	*1/2 mi.*	*8 mi.*	*Loss*	*Relative shape after 8-mile transportation*
Granite pre-Cambrian	12.2	1.2	90.2%	15% less rounded (more angular)
Vein quartz	11.4	1.4	87.8%	0% no change in angularity
Quartzite Paleozoic	17.6	2.0	88.7%	24% more rounded
Basalt Triassic	18.2	1.7	90.6%	27% more rounded
Sandstone Triassic	14 3	1.4	90.2%	29% more rounded

The specific area of outwash plain covered is 8 miles long from northeast to southwest and 4 miles wide. It represents a deltaic deposit of brown and reddish sand, which is known to be 41 feet thick at Park Avenue and East Fifth Street in Plainfield and over 15 feet thick at High Street and Mountain Avenue in Bound Brook. This deposit heads into the terminal moraine near the point where the latter crosses the first Watchung basalt ridge in the vicinity of Scotch Plains, northeast of Plainfield. Its most southwesterly outcrops are at Bound Brook, 8 miles southwest of Plainfield. Just beyond Bound Brook, it terminates against another alluvial deposit which is 2 miles wide and is composed of basalt fragments which were swept out of the watergap at Chimney Rock, in the first Watchung ridge, by a separate current of swiftly flowing waters which also came from the melting Wisconsin glacier.

It was noted in the field that, in the basal part of the main sand outwash

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[*] The series of papers appearing under this general heading, were presented at the meeting of the Section on October 3 1940

[†] Polytechnic Institute of Brooklyn, Brooklyn, N. Y.

[‡] Method used by the petroleum geologists.

deposit of the outwash plain northeast of Bound Brook, there is a heavy bed of four- to six-inch pebbles (10 to 40 feet down), which marked two early stages of deposition. Above this horizon, a deep excavation made recently at Bound Brook showed 5 feet of red-brown sand from local Triassic sources, lying *above* ten or more feet of brown sand, evidently derived from more distant sources. Samples of the pebbles studied in connection with this report were obtained from numerous cellar excavations for dwellings, which were made during the time of this investigation. The principal pebble-bearing layer is generally about 2 feet below the subsoil of the area. Averages of measurements on a number of pebbles of each kind were taken in each case. In TABLE 1 are shown the maximum differences between the pebbles at the head and at the foot of the outwash sand deposit. It will be noted that the size of the pebbles at the outer margin of the outwash plain averages 10 to 15 per cent of that of the pebbles near its head.

STRUCTURE-DRAINAGE RELATIONS IN THE APPALACHIAN MOUNTAINS

By HENRY D. THOMPSON*

The Problem of Appalachian Drainage Reversal. It is generally agreed that, following the Appalachian Revolution, the divide between the east-flowing and the west-flowing streams of the Appalachian Mountains was located somewhere east of the present Great Valley in the crystalline rocks of Old Appalachia. Overthrust faults on the west side of the Blue Ridge indicate that this old land was elevated more than the geosynclinal area to the west. Keith (1928) says that "... these overthrusts descend toward or into the great Blue Ridge anticline which exposes the most ancient rocks and forms the chief line of uplift of the Appalachians." As late as Upper Triassic time, the main divide was, probably, still east of the area of Newark deposition. Kummel (1940) states that "... the great bulk of the feldspathic and micaceous sandstones that make up so much of the Newark rocks must have come from higher lands that still existed to the southeast."

Southwest of Roanoke, Virginia, where the Blue Ridge is broad, the divide is still in the crystalline rocks, though it has shifted some miles westward. Northeast of Roanoke, the Roanoke and James head in the Ridge and Valley province west of the Blue Ridge. Farther northeast, the Potomac, Susquehanna, and Delaware and the west branches of the Hudson draw their headwaters from far into the Appalachian Plateau, a distance of eighty to a hundred miles from the original divide.

The problem before us then is to determine the most probable sequence of events and the method by which the drainage divide shifted from its presumed original position to that which it now occupies. Over a belt that is broad in the north and narrows southwestward, the streams that formerly ran westward now flow eastward. How did this reversal take place?

Various Hypotheses. One of the first serious attempts to explain the reversal of Appalachian drainage was Davis' classic paper of 1889. In this paper, he expounded many principles of stream erosion on and adjustment to folded structure that have stood the test of time. However, the conception of the manner of reversal of the original northwest drainage has received little support from subsequent thought. By restoring the original topography of the uneroded folds, a task in itself hazardous, Davis pictured the original drainage of Pennsylvania as consisting of a master northwest consequent, the Anthracite River, and a number of longitudinal consequent tributaries. The master Anthracite was then reversed in its course by diastrophism—the Newark depression—and later dismembered by capture to produce the present drainage. It is highly doubtful that the Newark depression was sufficient to reverse bodily the great Anthracite for any considerable distance. Furthermore, if such a reversal did occur, it is difficult to believe that the great river would have been dismembered by lesser streams that had no advantage of shorter distance to the sea.

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In 1895, Willis proposed that the southeast streams in the Appalachians had taken their courses on the eastward-tilted Kittatinny (Schooley) peneplane and had become superposed across the structure from a deep mantle-rock cover. Many of the streams that were thus let down onto resistant rock were unable to continue in their courses and were captured by their larger or better located neighbors, leaving wind gaps as evidence of their former courses. This hypothesis gained considerable acceptance and is still preferred by the U. S. Geological Survey. Many workers doubt, however, that the Kittatinny (Schooley) peneplane was sufficiently well developed or that the bed rock had a mantle sufficiently thick to allow regional superposition.

With these objections to Willis' hypothesis in mind and also recognizing the remarkable adjustment of the streams to structure, Johnson (1931) proposed a different hypothesis of regional superposition. To allow more time for adjustment, Johnson postulates superposition on the Fall-Zone peneplane. To provide a sufficient covering blanket, he had that peneplane veneered with marine sediments. This hypothesis of regional superposition of the southeast drainage from a coastal-plain cover on the Fall-Zone penepeane has attracted a number of adherents and has been adopted by the authors of some of the leading textbooks on historical geology. Objections to it, however, are serious. Stratigraphic and paleontologic evidence oppose the assumption that the Atlantic Coastal Plain formerly extended far inland from its present position. Land areas sufficient to supply the sediment for this extended coastal plain have not been located. Further, there is no convincing evidence that a well-developed Fall-Zone peneplane ever extended far inland. In fact, it is difficult to visualize how a single peneplane could have developed across the original asymmetrical divide.

Meyerhoff and Olmstead (1936) make a different approach to the problem. They postulate that, during the Appalachian Revolution, the axis of greatest uplift and, therefore, the original drainage divide, were far west of the crystalline area. From that original divide, the east-flowing streams have since pushed their headwaters farther westward by capture of the opposing headwaters. As yet, Meyerhoff and Olmstead have not produced sufficient evidence to gain wide acceptance of their belief that the original drainage divide was located west of the crystalline area.

Recently, deBethune (1948) presented a proposal which is a modification of Willis' old hypothesis. According to deBethune, the Schooley peneplane, on being uplifted, was warped in various directions. The streams took their consequent courses on that warped surface and have continued without appreciable change of pattern or direction to the present time. The most obvious criticism of this hypothesis is that of the uncertainty of recognizing the upwarps and downwarps of the Schooley peneplane. It is possible that the sites of some or all of the longitudinal lowlands, which deBethune maps as Schooley downwarps, existed as relatively high ground during Schooley time and that the topography has since been inverted. Present slopes conform with present drainage and are not necessarily in the same direction as those of Schooley time.

Hypothesis of Progressive Piracy and Local Superposition. The hypothesis here advanced accounts for the southeastward drainage of the Appalachians by normal processes of headwater piracy along an asymmetric divide. In other words, the original west-flowing streams have been turned to the east by capture. In this manner, the main watershed has slowly migrated westward from its early location in Old Appalachia to its present position. As a result of, and concomitant with, the westward shifting of the divide, the general regional slope has been reversed from a westward to an eastward direction.

In extending their headwaters westward, the southeast-flowing streams have been greatly influenced by structure. In general, they follow zones of least resistance, seeking out weak-rock formations and local weaknesses in the resistant ones. The factor of distance, too, has been important, for long, low-gradient streams are at a disadvantage in competition with those having short, steep courses. The particular courses of the transverse streams, then, are often the result of compromise between distance and structure. Evidence supporting this hypothesis is discussed in Thompson (1936, 1939, and 1949).

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MANHATTAN SCHIST AS A FOUNDATION FOR STRUCTURES

By JACOB FELD*

In the middle 1920's, a large investment firm in New York promiscuously advertised their offer of building mortage bonds under the slogan "as sound as the bedrock of Manhattan." That firm became insolvent before 1930, when some unforeseen maladjustment of our economy upset the soundness of their mortgage values. Their slogan, however, was, unknowingly to be sure, a correct statement. The bedrock of Manhattan is often an expensive and unpredictable foundation for structures.

With the exception of some small exposures of Inwood limestone near the Harlem River, almost the entire area of Manhattan Island is covered at various depths below the present surfaces with a coarse quartz-mica schist identified as Manhattan schist. From a geological point of view, this rock has been described in two early papers presented before the New York Academy of Sciences, in 1908 by Charles P. Berkey[1] and in 1914 by Charles Richard Fettke[2] as well as in many other papers and books. The purpose of this paper is to discuss the engineering evaluation of the Manhattan schist as a foundation support.

Even in the densest Manhattan schist rock, there are numerous, irregular streaks and intrusions of various colors and minerals. Fracture is easy but along uncertain planes. Drilling is not too definite, holes very often deviating from straight lines, and care must be exercised to prevent the drill from sticking. Most of the rocks exposed can be wedged and peeled. Preparation of a level surface is almost impossible. Split faces are slippery because of the large amount of mica exposed, usually gray or pearly in color. The rock pulverizes under traffic into a dust which becomes a slippery mud when wet. In construction operations, this property often necessitates a cover coat of sand or cinders to eliminate

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the dust during dry spells and to provide traction for vehicles during and after rains.

The maximum permissive load to be imposed on the exposed soils, including rock, is governed by the requirements of the New York City Building Code, which is Chapter 26 of the Administrative Code.

The code of 1938 (Section 377.0), similar to earlier editions, except that the allowance for "medium rock" was 15 Tons, permitted the following values:

Class 1—Hard sound rock —40 tons per sq. ft
Medium hard rock—25 tons per sq. ft
Class 2—Hard-pan overlying rock—10 tons per sq. ft.
Soft rock — 8 tons per sq. ft.

There were no definitions of the terms used, nor any tests for identification of the different rock conditions. General practice required that a steel bar dropped on rock considered hard emit a clear ring and that hard, sound rock have no seams.

As a result of an 18-month study by a special committee of 12 foundation experts chosen by the City Council on recommendation of various technical groups (of which committee the writer was a member) to revise various parts of the provision on foundations, with special reference to the sections on the use of piles, the Building Code was amended on November 17, 1948. The present maximum allowable presumptive bearing values on rock are:

Class 1—Hard sound rock —60 tons per sq. ft.
Class 2—Medium hard rock —40 tons per sq. ft.
Class 3—Hard-pan overlying rock—12 tons per sq. ft.
Class 4—Compact gravel —10 tons per sq. ft.
Class 5—Soft rock — 8 tons per sq. ft.

Definitions for identification were also added, as follows: "Hard sound rock is rock such as Fordham gneiss, Ravenswood gneiss, and trap rock in sound condition, with some cracks allowed. Medium hard rock is rock such as Inwood limestone, Manhattan schist, and massive serpentine, with some cracks allowed and slight weathering along cracks. Soft rock is rock such as shale, decomposed serpentine, decomposed schist, or decomposed gneiss, with some disintegration and softening and with considerable cracks allowed."

The Code makes the assumption that the exposure of rock in foundations, covered with concrete of the footings, is unaltered with age. The percentage of rock, especially in the schist areas, which can be classified as better than "soft rock" is extremely small. This is fortunate, because of the tendency of the schist to soften and disintegrate even after being sealed by concrete. The time required for such change, in the built-up areas of the city, is surprisingly small. Exposure of footings supporting buildings is not uncommon. Development of adjacent areas for new buildings and for subway structures is frequent, and a part of such construction is the frequent exposure of footings founded on rock. Underpinning of such footings to lower levels is often necessary, because the rock below the footings is not sound. Normally, it is assumed that these footings had been placed on unsound rock even if the records indicate that it was intended to found on sound rock. During the construction of the 6th Avenue Subway, several buildings were underpinned to lower levels through disintegrated rock. In one case, the supervising engineer of the construction of the foundations (in 1917) personally examined the exposure in 1936. His original field records were still available and his notations of "hard, ringing rock" for each of the footings left no doubt that the rock had altered completely in 20 years. The disintegrated rock under the footings was removed by hand shovels and picks, after temporary shoring of the columns, to as much as a 25 foot depth before sound rock was again encountered. The records of the old 6th Avenue Elevated Line, then over 50 years old, indicated sound rock levels much higher than were actually found when the footings were removed. Even the rock under the footings of some monumental structures along 6th Avenue, less than 10 years old (in 1936–8) could easily be removed by one's fingers.

There is definite indication that mica schist, under superimposed loadings of approximately 25 tons per square foot, changes physically in a matter of 10 to 20 years. The altered rock is not an unsafe foundation if there are no cavities or seams into which the compressed material, more plastic than solid, can flow. Danger exists only if the footings are not recessed into pits (the normal procedure is to cut a pit at least 12 inches deep into sound rock for a proper footing) or when new excavations remove the lateral support of the rock just below the concrete base.

Definite indications that mica schist, when operated upon by either surface or tunnel excavations, soon becomes a discontinuous body are also often encountered. Three examples are given from the 6th Avenue Subway construction experience. Near 41st Street, the excavation uncovered the roof of the Queensborough Subway, which had been tunneled in rock with some timber centering supports. The timber was found encased in cement mortar which had been forced upwards as a grout to seal the cavities in the rock. The rock in the entire area was removed in 1937 by means of a power shovel of small capacity with no drilling or blasting. Actually, the City refused to pay for the excavation as "rock" and classified it as earth. The engineers who had supervised the earlier tunnel operation, about ten years earlier, testified that the rock in the tunnel had been seamy and blocky but hard and dense.

At the time of subway construction in 6th Avenue near 44th Street, the Hippodrome had been demolished and the old street vaults in which the circus animals had been stored up to about 1935 were no longer in existence. The subway, in the blocks from 43rd to 46th Streets, was constructed as rock tunnels with varying amounts of mica-schist cover over the tunnels. The rock opposite the Hippodrome site was among the densest of all the rock exposures and there was some 20 feet of thickness between the roof of the tunnel and the floor of old vaults. Yet samples of air, taken inside the tunnel at that location for dust-count determination, showed large concentration of bacteria and the odor was definitely reminiscent of animal refuse.

Some 500 feet further north along 6th Avenue, in the excavation of a shaft for spoil removal from the tunnels, normal soil and disintegrated rock were encountered for 25 feet below street level, and mica-schist rock had been drilled and blasted for another 10 feet in depth when it was noticed that the pit bottom was unduly hot. Further excavation was in quite soft rock and, for the next 10 to 15 feet of depth, steam vapors continuously fogged the pit. Rock temperatures were above 150° F., and the excavation was apparently through a subterranean pocket of steam condensate flowing from a public utility steam system located some 40 feet away horizontally and with at least 20 feet of undisturbed bedrock penetrated by the steam condensate. Leaky sewers also do their part in speeding up disintegration of the rock.

The lack of uniform surface patterns in mica-schist rock areas make subsurface borings, soundings, and even open test pits of little value as a guide for predicting depth of rock beyond the actual location of the test hole. In the 6th Ave. Subway exposure, 100 feet wide, it was not unusual to have the rock vary 40 feet in depth at any one section. The surface was irregular, with many reverse slopes. A recent rock exposure in the block at the N. E. corner of 98th St. and 3rd Ave. showed rock levels varying over 30 feet within a distance of 50 feet. In many cases of piles driven to bearing on rock, piles in one cluster will vary in length by as much as 20 feet. In the excavation along 48th St., east of 12th Ave., with the street rising some 15 feet upward to the east, the rock surface at the higher land area was found 20 feet lower than at the 12th Ave. side.

It is recognized in the excavation for foundations to bear on Manhattan schist that the footing levels will vary over a considerable range. Since it is not good practice or good engineering design to have adjacent footings on rock differ more in depth than the clear distance between them, to avoid lateral transfer of loads from one footing to another, complete rock exposure is substantially required before any of the footings can be completed.

In addition to the variations in depth of the rock surface, another difficulty arises from the nature of the seam structure. In most areas the dip and strike are fairly constant, but ragged edges, some 30° from the vertical, almost always appear in the exposed footing bottom, which the contract requirements show and require to be substantially level. Manhattan mica schist cannot be prepared in level beds. The rough exposure is not theoretically detrimental to the carrying capacity of the footings; but it looks bad and has caused more cost and time wastage than any other factor in local foundation construction. Drilling, chipping, grinding, and any other means of rock removal only result in further disturbance and loosening of rock segments; yet the custom of specifying "level" beds in rock foundations is difficult to overcome.

Large volume displacement is another characteristic of the mica schist. Sometimes it results in a slide such as caused the movement of some 6000 cu. yds. of rock partially to fill the excavation of the 8th Ave. Subway cut in one block, with the loss of all bracing, decking, and other construction materials. In the approach to the Battery Tunnel, large volumes moved out of position and distorted steel bracing which had been placed to maintain the excavation surfaces. These movements do not occur immediately after exposure, but sometimes a year or more later, as percolating waters alter the internal resistance along seams or chemically alter the internal rock structure at seams or contact surfaces.

In several localities, the schist is exposed as a rock cliff. Structures on the street at the top of the cliff are often caused to crack when development of the land at the base of the cliff removes some of the resistance to rock movement. The construction of foundations along Jerome Ave. in 1929 removed enough of the lower rock to cause the almost vertical sheets carrying the rear walls of the building on top of the cliff on Anderson Ave. to buckle and displace the rear of the building. Some 150 families were vacated from the upper building when the rear-yard wall collapsed, and a series of ties into unaffected rock had to be built to stop any further outward movement. As a result of this occurrence, all later building construction on top of the cliff was provided with ties and deep steel dowels, so that the rock sheets were practically sewn together before any footings were built thereon.

A less frequent defect in the rock is the existence of large cavities. When discontinuity in rock volume is exposed at the rock surface and probings indicate considerable depth of soft filling, concrete arches are often sprung across the chasm. Such a solution was necessary at 200th Street, east of the Grand Concourse, where a quicksand-filled gap, less than 20 feet in width, was found in an otherwise sound rock area. A more serious condition was encountered on St. Nicholas Terrace near 155th Street, in the subway construction. Several buildings had been underpinned to rock and the excavation had progressed to a depth of some 35 feet, mostly through solid rock, when an underlying bed of loose material was uncovered. This required the further underpinning of the rock to lower rock. The gap in rock continuity was several feet in height, over 100 feet in length, and some 25 feet below the first-encountered rock surface.

Manhattan schist is and can be used as a foundation for many structures. It is, of course, far superior to many of the soft clays and silts found elsewhere. For its use as a permanent support, some study must be made of the nature and possible changes of the rock with age in the area of the site being developed. It is as a guide to some of the possible difficulties which may be encountered that this paper is presented.

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A STUDY OF MINOR BEACH FEATURES AT FIRE ISLAND, LONG ISLAND, NEW YORK

By M. B. ROSALSKY*†

Fire Island, an offshore bar, constitutes the southern border of Great South Bay and is located off the coast of south central Long Island. It is an ideal location for the study of beach features since it is a long straight beach with fairly uniform conditions of deposition and erosion.

Examination of the literature reveals that many minor beach features have been little studied, and that some, such as overwash marks and sand drips, have previously not been studied at all. Studies of present-day beach features have helped to explain much in the geologic record to such investigators as Fairchild,[1] Clarke,[2] Powers,[3] Evans,[4] and Peabody.[5] Further study of such forms may throw additional light on the geologic past. Ordinarily, these features are ephemeral and are destroyed by the rising tide, but they have been preserved in the geologic record whenever they were formed at exceptional tides or on a rising coastline.

A Beach Cycle. Minor beach features are commonly formed in association with the development of a beach cycle shown in FIGURES 1, 2, and 3. The foreshore is built up during calm weather, reaching equilibrium on Fire Island at slopes of from about 4° at the lower to about 12° at the upper foreshore (see FIGURE 1). The foreshore is the part of the shore which lies between the ordinary high and low water marks and is daily traversed by the oscillating water line as the tides rise and fall. The backshore is the portion of the shore covered by water only during exceptional storms. FIGURE 2 reveals that, if stormy conditions occur, the foreshore is eroded down to a more moderate slope and is thus separated from the backshore by a storm-formed cliff (beach scarp). With the slackening of the storm, the foreshore may build up its profile steeply, so that the seaward margin of the backshore is closer to the ocean (see FIGURE 3). The backshore terrace, thus formed, now stands higher than and closer to the ocean than the surface which, in FIGURE 2, was the upper foreshore with its gentle storm-eroded slope. Between the backshore terrace and the beach scarp there is, thus, a depressed area into which water can enter only at high tide. This area, CD of FIGURE 3 (which, in this case, is of the order of thirty to sixty feet wide and several hundred feet in length, parallel to the shore), may, if it contains water, be called an overwash pool. This term follows the usage of D. W. Johnson,[6] namely, that water which has overridden the backshore terrace and flows down the landward slope is overwash. The suggestion to use the term "overwash pool" comes from a personal communication to the writer from Francis P. Shepard.

Finally, if calm weather is maintained long enough, deposition will continue on the foreshore so as to restore the original profile of FIGURE 1. It should be noted that the beach scarp is only three to five feet high but seems much higher because of vertical exaggeration. The backshore area is sometimes called a berm. Therefore, FIGURE 3 could be regarded as a double berm. Sometimes an overwash pool develops on the backshore shown in FIGURE 1 during exceptionally high tides.

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† The writer is very grateful to Dr. Francis P. Shepard for his helpful criticism and comments.

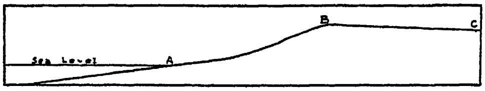

FIGURE 1 Calm weather foreshore—AB, backshore—BC

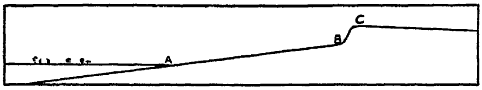

FIGURE 2. Foreshore eroded down by storm—AB, beach scarp—BC.

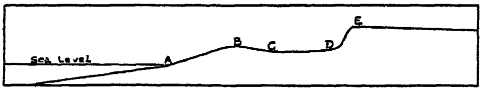

FIGURE 3. New foreshore—AB; backshore terrace—BC; area where overwash pool is formed—CD, beach scarp—DE.

Preliminary Survey of The Minor Features Formed. Starting at the ocean, the first minor features to be discussed are the swash marks on the foreshore. They are thin arc-shaped lines of sand, deposited on the foreshore by the thin sheet of water (the swash) and derived from a broken wave which glides up the foreshore slope. Then, overwash marks (a term proposed by the writer) are formed on the backshore terrace. They are features formed by the absorption of the overwash into the sand on the landward slope of a backshore terrace. At the border of the backshore terrace and overwash pool, straight or rounded deltas are formed. In this area, miniature cliffs and terraces may form on the foreset slope of the deltas. Also, channels are occasionally cut in the deltas when overwash, traveling slowly down the gentle slope into the overwash pool, is suddenly accelerated by the steep foreset slope. In but a few seconds it may scour out a channel in this slope and, having reached a low gradient below in the overwash pool, deposit a fan.

In this same area, sand drips (a term also proposed by the writer) are sometimes found (see PLATE 1, A). These are rounded masses of sand, formed when the water carrying the sand is suddenly absorbed into the underlying sand. On the floor of the overwash pool are found current, oscillation, and meta-ripple marks. Just landward of the overwash pool is located the beach scarp of laminated sand. On the beach scarp can be seen such features as faulting, cross-bedding, ledges with talus accumulation, channels, columns, and pinnacles.

Swash marks. The swash carries with it sand and flakes of mica by flotation,[7] which it deposits to form swash marks. Observation of advancing swash reveals that it consists of many long narrow prongs of water bearing little resemblance to the swash marks of the beach. However, the swash commonly advances up the beach at an angle and sweeps around in a parabolic orbit at the point of farthest advance. This has the effect of evening off the swash mark into the typical broad curve, concave in a downslope direction. As the tide ebbs, swash marks are formed farther and farther downslope. The lower marks cut into the upper ones, partially destroying them on account of the haphazard position of a higher crest on the wave.

At low tide, a broad belt of swash marks is exposed to view. These concave, downslope, swash-mark remnants remain, till the next tidal advance, as a record of the high crests of all the largest waves. On the steeper upper foreshore, the swash marks are close together, while on the more gently-sloping lower foreshore the marks are more widely separated. This should be the case since, for every inch of the lowering of the tide, a greater horizontal area is exposed to the same number of waves on a gentle rather than on a steep slope.

The writer carried out a study of this relationship. A number of traverses, ten feet apart, were made at Kismet Beach, Fire Island. Each traverse was divided into ten-foot courses. The average angle of slope of each ten-foot course was noted, as were the number of swash marks for the course. The work was done from an hour before low tide to an hour after, exposing to view the upper and lower foreshore very well.

A graph was constructed comparing the number of swash marks per ten-foot course with the angle of slope of the course (FIGURE 4). The graph shows clearly, although the data are somewhat scattered, that the steeper the slope the larger the number of swash marks found.

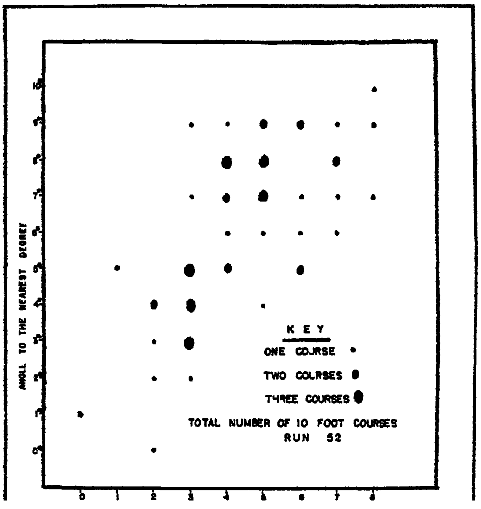

FIGURE 4. Relationship between the number of swash marks and the angle of slope of the beach.

Overwash Marks. When the backshore terrace is overridden by the overwash only at the highest tide, the water may seep into the sand without forming an overwash pool. The overwash marks formed have not been described in the literature (PLATE 1, B). Overwash marks contrast strongly with orthodox swash marks, for overwash marks are composed of coarser material and are thicker and wider than swash marks. Swash marks have a concave pattern, facing downslope and of wide arcuate form, while overwash marks have a convex pattern, facing downslope and consisting of narrow tonguelike peninsulas (PLATE 1, B).

The aforementioned phenomena may be explained in the following manner. It takes water with a considerable velocity to override the backshore terrace. This water is carrying the larger grades at the time it is absorbed into the sand. These are the grades which make up the overwash marks. The swash also carries these same grades, but the backwash carries them out again, leaving only the fine material carried by flotation and deposited as the swash marks. The overwash marks, consisting of these larger grades, are naturally thicker than the swash marks. Overwash marks are wider than swash marks because most of the material is swept in and deposited. The swash, on the other hand, carries in material, much of which is carried out by the backwash.

The multitude of tonguelike peninsulas of which the overwash marks consist is the shape typical of any advancing swash, exaggerated by the fact that the irregular, broken wave has traveled a longer distance over the sand and consequently has been somewhat restricted into channels. There is no parabolic motion possible to round off the overwash marks. Instead, the actual tonguelike form is deposited on the sand as the water is absorbed.

Swash marks are found from the high-tide line to the low-tide line in their haphazard, cross-cutting, concave downslope pattern. Overwash marks, on the contrary, advance farthest downhill at the highest tide. With the ebbing of tide, they override the backshore terrace to a lesser amount and so are found further up the slope. There may be comparatively few of them if the overriding of the backshore terrace occurs only at high tide. Their position is not governed entirely by the impetus of the original erratic wave. Rather, slope control asserts itself more strongly than in the case of swash. Therefore, crosscutting of one overwash mark by another is not common. This is so because the water running off the gentle downslope tends to localize itself into channels. The later overwash, which does not go so far, still keeps in the same channel, reducing a crosscutting tendency.

Overwash Pool Deltas. When the backshore terrace is overridden by the overwash an hour or more before high tide, an overwash pool may form. The oncoming overwash meets the still water of the pool and deposits a delta at the margin of the pool. But the overwash pool is steadily getting larger, with accessions of water all along its seaward margin. Thus the next time the overwash carries sand into the pool, it cannot carry it over the original delta and deposit it on the foreset slope, for this delta is now in water several inches deep and several inches from the overwash-pool shoreline. Thus, a new delta is built at the new margin of the pool, roughly parallel to the first one. As the pool progressively increases in size, more and more deltas are built and drowned. Sometimes as many as six may be seen. The fact that each delta and foreset slope is an entity in itself, rather than a blending, illustrates that the overwash pool does not enlarge at a steady gradual rate. Rather, it has a series of sudden increases in size, as the result of periodic floodings by overwash. Generally, these deltas rim the pool and are parallel to the margin.

Sometimes, however, the overwash is concentrated into channels on its way down to the overwash pool. In this case, a series of rounded deltas is formed, each apexing at a channel. With the enlargement of the pool, new systems of rounded deltas are formed, parallel and concentric to the first, giving a very striking pattern (PLATE 1, C).

Miniature Cliffs and Terraces. At the height of high tide, quite a large delta may develop, since the overwash-pool water level is at its maximum in a state of rough equilibrium for about an hour. This large delta, whose foresets may be six inches high, is gradually exposed to view as the water level declines with the ebbing tide. The water level declines, since less and less overwash enters the pool and there is a steady rate of absorption of water into the underlying sand. Miniature cliffs about ¼ inch in height and terraces about ¼ inch in width (PLATE 2, A) are cut in the foreset slope of the large delta by the wavelets on the surface of the overwash pool. These cliffs and terraces have been only briefly alluded to in the literature.[5]

The declining water-level stages are revealed by a long succession of cliffs and terraces in a step-like pattern. The fact that there are cliffs and terraces, rather than a smooth slope, shows that the water level declines with a number of stand-stills, rather than with a gradual lowering. A fairly long period without entering overwash is probably a time of lowering of the water level, with a stand-still occurring with a fresh incursion of overwash. At the latter time, the cliffs and terraces are formed. At low tide the miniature terraces bordering the foreset slope stand out like contours. These features can develop on any shoreline of an overwash pool. If the water level happens to intersect ripple marks, cliffs and terraces are formed on them.

Sand Drips. When the overwash enters the overwash pool at the next flooding tide, it may form channels in the foreset slope, as was discussed previously. When sand is carried only an inch or two in the channel and then absorbed, however, peculiar rounded sand forms result (called by the writer "sand drips"). These sand drips may have a concentric pattern if there is a succession of entering overwashes which are channelized (see PLATE 1, A). The water in the center of the channel flows down into the overwash pool, but the water on the sides of the channel which overlaps the divides is absorbed to yield a sand drip there. With a number of such wettings, a concentric pattern of sand drips is formed. For the most part, these features are of very short duration, for, generally, overwash of great volume at higher tide destroys all evidence of them. The literature makes no mention of these features.

PLATE 1, A shows the ocean with breakers in the background and the overwash, in many prongs, gliding down the landward slope of the backshore terrace. In the

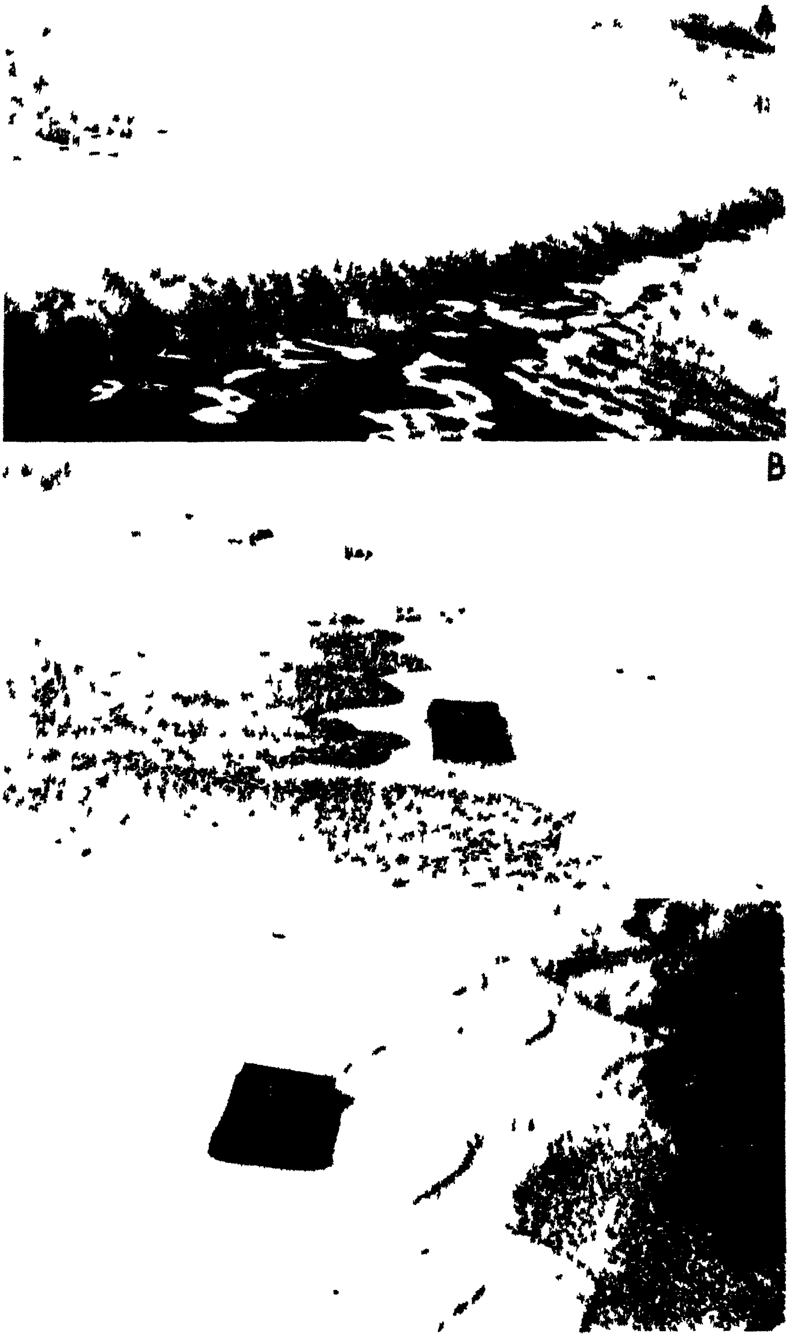

PLATE 1

A Concentric sand drips on margin of overwash pool at Kismet Beach Fire Island
B Wide overwash marks of coarse material showing narrow tongue like peninsulas On landward slope of a backshore terrace Near Fire Island Lighthouse
C Series of parallel concentric deltas at border of overwash pool near Fire Island State Park

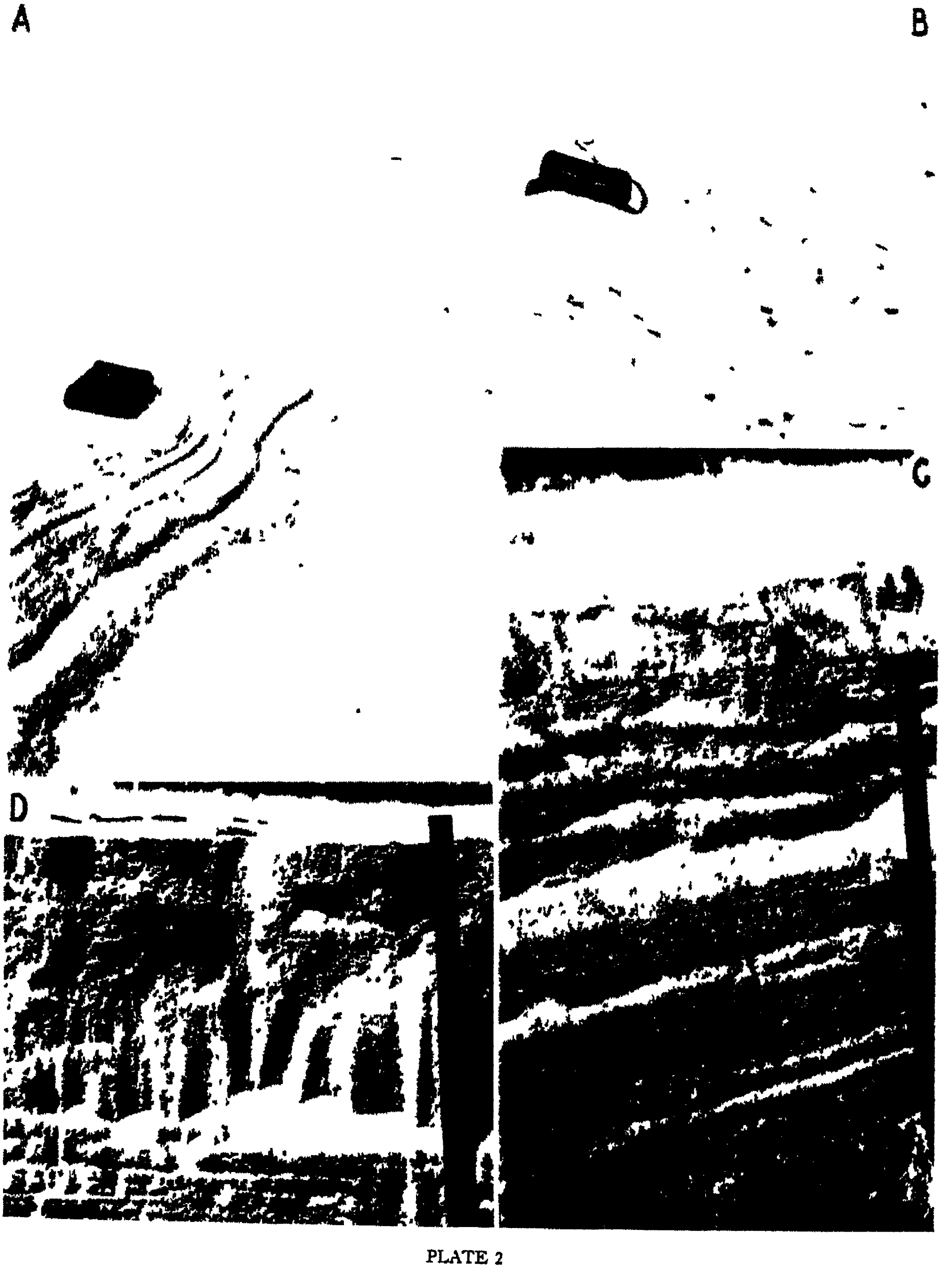

PLATE 2

A. Cliffs and terrace on foreset slope, seaward margin of overwash pool at Kismet Beach, Fire Island
B. Control exercised on position of small current ripple marks by position of meta ripple marks at Fire Island State Park.
C. Indurated horizontal laminations with talus accumulation on projecting ledges, at Kismet Beach, Fire Island
D. Channels and fluted columns on scarp face at Kismet Beach, Fire Island

foregound can be seen the sand drips separated by intervening channels and bordered by the waxing overwash pool.

Oscillation, Current, and Meta-Ripple Marks. The overwash which overtops the backshore terrace on its way to the overwash pool, when large in volume, has waves on its surface. When the overwash reaches the water surface of the overwash pool, the waves continue right across. This is, however, not true of the water particles. For example, the motion of foam is almost entirely checked at the margin of the pool, revealing that there is but little water motion. There is, rather, a rise in the level of the pool with the addition of the water. Deltas at this point also show that there is an absence of water motion. It appears that the waves in the overwash, which were waves of translation, are changed into waves of oscillation when they reach the overwash pool. This is also confirmed by the presence of oscillation ripple marks parallel to the long axis of the overwash pool, that is, parallel to the coast. These have the symmetrical slopes, sharp crests, and wide, rounded troughs so typical of symmetrical oscillation ripple marks. As Evans has pointed out,[9] a truly symmetrical oscillation ripple mark does not travel and is definite proof of a wave of oscillation.

A curious feature is that, as soon as the overwash reaches the pool, a wave form starts to rebound toward the ocean up the 1° to 5° slope of the backshore terrace. For, as the water reaches the pool and is poured violently into it, a wave is formed just as if a stone were thrown into a lake. This wave of oscillation travels outward in all directions, including a slow motion up the backshore terrace toward the sea, despite the fact that the water particles of a continuing overwash are still actually traveling downslope into the overwash pool.

As the overwash adds more and more water to the overwash pool, the water is escaping from the pool in two ways. Some of it percolates through the sand and thence back to the ocean, which is at a lower level; and, if a great volume of water has been added, some of it travels as a current parallel to the shore until it finds a low place in the backshore terrace which it flows through in its journey back to the sea. A few minutes after this current has been established, current ripple marks are formed with typical asymmetrical slopes and rounded crests. The steeper slope is on the down-current side.

Sometimes, there are developed in the overwash pools larger ripple marks, five to six feet from flat crest to crest and two to three inches high. Such large ripple marks were called "meta-ripples" by Bucher.[10] On these large meta-ripple marks, small current ripple marks may develop. Kindle,[11] describing this condition, stated that the small ripple marks were formed near the end of the ebb tide, when the current was weaker and held a smaller volume of sediment than at higher tide. He stated that both sets are parallel and trend at right angles to the direction of the current. The writer also found the small current ripple marks trending at right angles to the direction of orientation of the large meta-ripple marks (see PLATE 2, B). Thus, in this case, the weaker current of the ebbing tide followed the troughs of the meta-ripple marks to form the small current ripple marks with their orientation at right angles to that of the meta-ripple marks.

Beach Scarp Features. Landward of the overwash pool may be found a beach scarp, as referred to in the discussion of the beach cycle. The position of the beach scarp on the backshore indicates that the sand in it is water-laid instead of wind-laid. This is confirmed by the presence of horizontal pebble beds which indicates water-laid deposition. For, as Evans points out,[12] an indication of water-laid material is the presence of isolated horizontal layers of pebbles. On the other hand, wind-laid material may, in some cases, contain individual pebbles of small size; but a collection arranged horizontally is never found within the body of the sand dune.

At high tide, the cliff may be undercut by the swash so as to have a considerable overhang. The undercut sand, in some cases, slumps down to form a talus. In other cases, the sand is fairly well consolidated and slumps down in blocks of from one to several cubic feet in size. The slump blocks may suffer some rotation, as seen from a study of the laminations. Sometimes, the slump blocks fracture in transit, revealing a faulting which is shown by an offsetting of the laminations.

The beach-scarp face, exposed to view, shows a lamination which is revealed by alternate layers of light-colored quartz sand and dark-colored sand rich in magnetite and ilmenite. This laminated sand is sometimes cross-bedded. In some cases, the laminae consist of light-colored sand exclusively, but the fact that it is laminated is revealed by wind action. For the wind can more easily remove certain poorly indurated laminae than the other, better, indurated ones, leaving the cliff with an etched face of horizontal layers, some projecting farther out than others. When there are a number of projecting horizontal layers of indurated sand, the loose, dry, falling sand from the less indurated layers is caught on each projecting ledge and forms a talus accumulation there (see PLATE 2, C).

When there is a lack of projecting horizontal layers of indurated sand, some sand which becomes detached from the general, less indurated scarp surface is carried vertically down along the scarp face by gravity and forms a fluted channel there. When there are many such parallel channels, the intervening "stream divides" remain as vertical columns on the scarp face (see PLATE 2, D).

The part of the channel deepest in the cliff becomes progressively wider because of the continued attrition by the downsweeping sand. Thus, two adjacent channels may meet behind a column, leaving it standing as a pinnacle.

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THE CRETACEOUS CRAB *RANINELLA TESTACEA* IN NEW JERSEY

By CECIL H. KINDLE*

The Cretaceous crab *Raninella testacea* Rathbun was described from its occurrence in the Ripley formation at Coon Creek, McNairy County, Tennessee. Several illustrations show the dorsal surface of the body with the characteristic small hexagonal surface markings. One illustration shows some claws on the ventral view, but the body surface from this view is not exposed (U.S.G.S., Prof. Paper 137, 1926).

In her 1935 monograph on "Fossil

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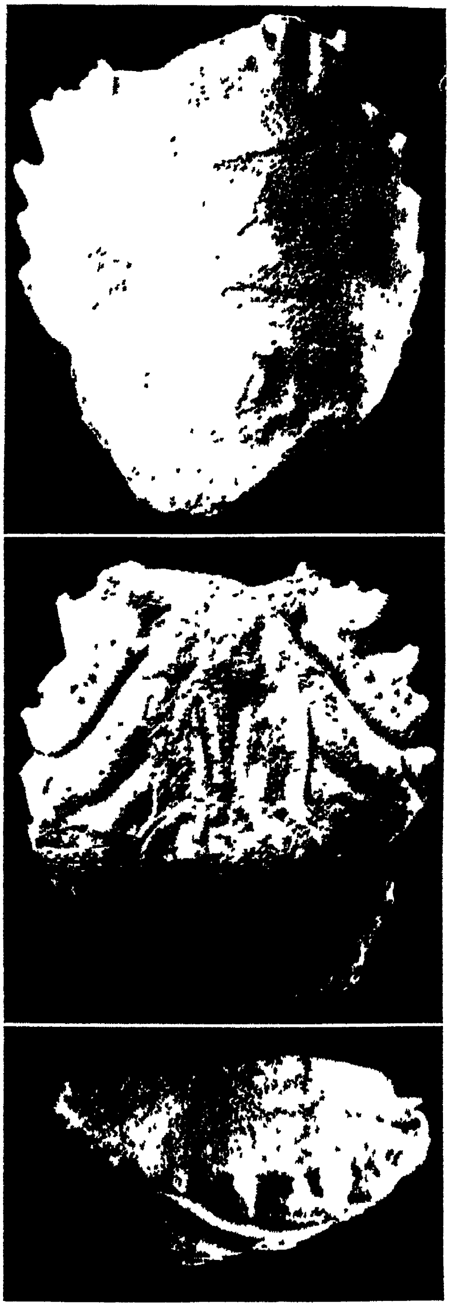

FIGURE 1. Dorsal, ventral, and side views of *Raninella testacea* from Atlantic Highlands, New Jersey. x 1⅓

Crustacea of the Atlantic and Gulf Coastal Plain," Mary J. Rathbun lists no other occurrence of this species, nor does she bother to reproduce its illustration again. No doubt the previous ones showed all that is known of this species.

In the course of field work near New York City with paleontology classes, a number of odd things have been turned up here and there. Among these was the body of a crab found last year by a student, Samuel Rosenbloom, at Atlantic Highlands, N. J. Among the most common fossils to be found at that locality are "crab claws" (*Callianassa mortoni*), so at the time of discovery we assumed the body was the same species as the claws, though we had never before encountered a crab's body. In going over the literature, however, we found this assumption to be erroneous. *Callianassa* is a burrowing shrimp with a soft body and only the two distal segments of the chelipeds are sufficiently strengthened to be preserved as fossils in the New Jersey Cretaceous sands and marls. Turning to Wade's "Fauna of the Ripley Formation on Coon Creek, Tennessee," we saw our find illustrated and described as *Raninella testacea* Rathbun.

This specimen from Atlantic Highlands originally showed only the dorsal surface. Working with a phonograph needle, we exposed a large portion of the ventral surface. No illustrations are known to the writer which show the ventral surface of this genus. For that reason also, this specimen from New Jersey is remarkable.

To Rathbun's list of crustacea from the Navesink marl of New Jersey (*Callianassa mortoni* and antenna of Palinurid) must be added *Raninella testacea.*

SECTION OF PHYSICS AND CHEMISTRY[*]

ELECTRONIC EXCHANGE AND OXIDATION-REDUCTION REACTIONS IN INORGANIC SYSTEMS[†]

By J. W. GRYDER[‡]

Oxidation-reduction reactions can be defined, in general, as reactions in which electrons are donated to an oxidizing agent by a reducing agent. On this basis, the transfer of electrons between two oxidation states of a single element can be considered to be a very simple form of oxidation and reduction. Such reactions are commonly known as electronic exchange reactions and can be studied by the use of radioisotopes. In this paper we shall concern ourselves only with reactions in which there is hope that no very great rearrangement of atoms occurs between the initial and final states of the oxidizing or reducing agent. Thus, exchange between arsenite and arsenate and the oxidation of oxalate by permanganate will be excluded.

The nature of oxidation-reduction reactions of relatively simple ionic species in solution has been the subject of considerable speculation.[1–3] The marked difference in the rates of these reactions can be illustrated by the extremely slow oxidation of thallous ion by ceric ion in sulfate solution and the very rapid reaction between ceric and ferrous ions in the same medium. It should be noted that both cerium and thallium form reversible electrodes. These results and other relatively scattered and unsystematic experiments have been explained variously as being the result of coulombic forces,[1] of the number of electrons donated or received[2] (*i.e.*, that reactions would be fast if the oxidizing agent requires the same number of electrons as the reducing agent is capable of donating, otherwise that it would be slow), and of the free energy change involved in the oxidation.[3]

A reasonable number of experiments have now been performed which enable a critical evaluation of these various hypotheses. TABLE 1 gives a summary of the results of some of these measurements as reported by various workers. $T\frac{1}{2}$ is the time for the reaction to be half finished when the reactants have concentrations of about 0.01 formal.

The fact that there are several exchange reactions which are slow indicates that rates are not necessarily fast if the number of electrons required for the oxidized and reduced species are equal. It is also to be noted that reactions such as thallous-thallic exchange, in which two electrons are involved, can be either faster or slower than one-electron reactions. Coulombic forces alone are not responsible for the rates of reactions, since thallous-thallic[1] exchanges much more slowly than cerous-ceric[5] when both are in perchloric acid. Finally, the fact that stannous-stannic[6] and cerous-ceric exchange relatively rapidly indicates that the oxidation potential is not the only factor affecting the rate, since in these cases the difference in potential of the initial and final product is zero, and yet the exchange rate is much faster than other reactions of these substances in which the free energy change is large negatively.

Since none of the hypotheses thus far advanced accounts satisfactorily for all the data available, an analysis of the exchange reactions which have been studied in detail was undertaken in the hope of elucidating the factors which determine the rates of oxidation-reduction in these relatively simple systems.

Several authors have studied various cobaltous-cobaltic exchanges and oxidation-reduction reactions[7] with the results given in TABLE 1. From these data, one might draw the conclusion that the free-energy difference between the final and initial states is of great importance in determining the rate of an oxidation reaction. Thus, the oxidation of $Co(en)_3^{++}$ by $Co(NH_3)_6^{+++}$, in which the free-energy change is negative, is faster than either of the exchange reactions; while the oxidation of $Co(NH_3)_6^{++}$ by $Co(en)_3^{+++}$, in which the change is positive, is slower than the other reactions. It should be stressed, however, that this principle may be used for prediction only when one is discussing the rates of reaction of very similar compounds. For example, one could make no reliable estimation concerning the relative rates of the reaction between ferric orthophenanthroline and cuprous chloride as compared with ferric chloride and cuprous chloride, because the amount of chloride attached to the oxidized and reduced species is probably not the same. Hence, large rearrangements must occur and the free energy change will contain a large entropy term. Nonetheless, the principle of large free-energy decrease leading to fast rates can probably be used in predicting the rates of cross reactions relative to the exchange reactions for such systems as the iron orthophenanthrolines, iron nitro orthophenanthrolines, and iron cyanide complexes. The results of experiments on these materials will be valuable in determining the importance of free-energy change. Unfortunately, no reliable data are yet available for these systems.

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[*] The Section of Biology held a meeting on Oct. 10, 1949, at which Dr. David Shemin, College of Physicians and Surgeons, Columbia University, New York, N. Y., presented a paper entitled *Biosynthesis of Heme: An Illustration of the Use of Isotopes in Biological Research*, illustrated with lantern slides. No abstract of this has been received.

[†] This paper was presented at the meeting of the Sections on October 11, 1949.

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TABLE 1

Oxidizing agent	*Reducing agent*	*T½*
Ce^{IV} (sulfate)	Tl^{I}	extremely long
Ce^{IV} (chloride)	Tl^{I}	very short
Ce^{IV}	Fe^{II}	instantaneous
Fe^{III} (sulfate)	Sn^{II}	long
Fe^{III} (chloride)	Sn^{II}	short
Tl^{III} (perchlorate)	Tl^{I}	several hours
Tl^{III} (nitrate)	Tl^{I}	shorter than in ClO_4^-
Tl^{III} (chloride)	Tl^{I}	becomes very short in high Cl
Ce^{IV} (perchlorate)	Ce^{III}	minutes
Ce^{IV} (nitrate)	Ce^{III}	minutes
Sn^{IV} (chloride)	Sn^{II}	minutes
$Co(NH_3)_6^{+++}$	$Co(NH_3)_6^{++}$	> 80 days
$Co(en)_3^{*+++}$	$Co(en)_3^{++}$	several hours
$Co(en)_3^{+++}$	Co^{II}	no reaction 3 months
$Co(NH_3)_6^{+++}$	$Co(en)_3^{++}$	minutes
$Co(en)_3^{+++}$	$Co(NH_3)_6^{++}$	> 500 days

* en = ethylenediamine

The rate of exchange between thallous and thallic ions has been measured and found to depend markedly upon the nature and concentration of the anion, as well as upon the acid concentration. The rate changes by a factor of 10^6 when chloride is added to the system in relatively small concentration. These data were interpreted as indicating that hydrolyzed species and chloride complexes were participating in the exchange. More recent unpublished experiments of Dodson show that the change in rate, when sodium perchlorate is added to a one formal perchloric acid system, is about the same as that caused by the addition of the same number of moles of perchloric acid. It is difficult to ascertain if this invalidates the previous interpretation of the acid dependence. One or both of the following may be operative: (a) the great change in the electrical properties of the medium directly surrounding the exchanging ions causes a large change in the rate; (b) the change in rate is the result of specific electrolyte effects on the activity coefficients of unhydrolyzed reactants and the transition complex. Regardless of which of these two is correct, it is evident from the chloride experiments that the exact nature of the ions will be of vast importance in determining the magnitude of the rate.

The last exchange reaction to be considered is that between cerous and ceric ions. In this case, we are chiefly interested in the order of the reaction. It was found that in nitric acid the reaction is first order in both cerous and ceric cerium, while in perchloric acid the dependence is first order on cerium IV at large ceric concentration but becomes zero order at low concentrations. The order with respect to cerous is always unity. These results are explained by the following series of reactions:

$$Ce^{IV} + Ce^{III} \xrightarrow{k} \text{exchange} \quad (1)$$

in competition with

$$Ce^{III} + S \underset{k_2}{\overset{k_1}{\rightleftarrows}} Ce^{III*} + S, \quad (2)$$

where Ce^{III*} is an excited state of Ce^{III}, and

$$Ce^{III*} + Ce^{IV} \xrightarrow[\substack{\text{relative to} \\ k_1 \text{ and } k_2}]{\text{rapid}} \text{exchange.} \quad (3)$$

This would lead to a rate equation

$$R = k'[Ce^{III}] + k\,[Ce^{III}][Ce^{IV}]. \quad (4)$$

This chemical evidence for the existence of an activated cerous material is in complete agreement with spectral and theoretical considerations. The electronic configuration of Ce^{III} is a $^2F_{5/2,\,7/2}$ and, due to spin-orbit and spin-spin interaction, it is expected that the ground state should be split into two levels[8] with separation of the order of magnitude of 1000 cm^{-1}.

By determining the emission spectrum of various solutions and salts of cerium, not including the perchlorate or nitrate, however, Kröger and Bakker[9] found a splitting of 1620 to 2145 cm^{-1} for this state, depending upon the material used. Their average value was 1900 cm^{-1}. This indicates that 10^{-3} per cent of the cerous ion is present in an excited electronic state at equilibrium at room temperature. The variation of the splitting with the compound used leads to the conclusion that the amount of material in the active state varies markedly. Finally, in view of the forbidden character of the transition, the rate of the excitation would almost certainly depend upon the nature of the medium. We cannot, however, state the quantitative relationship of this dependence.

Another principle arises from these arguments, *i.e.*, the rate is dependent upon the electronic excitation of the ions involved. It is not at all surprising that the electronic excitation is important, in view of the fact that one would expect the ease of electron transfer to depend upon such variables as the overlap of orbitals in ions which are in close contact, the spin characteristics of the electrons in the ions involved, *etc.*, all of which are in turn dependent upon the electronic state of the ions.

TABLE 2

Exchange reaction		*Medium*	*ΔS in e.u.*
$Co(en)_3^{++}$	$Co(en)_3^{+++}$	.98 ionic strength	−18.8
		1.98 " "	−13.7
Tl^{I}	Tl^{III}	6 formal $HClO_4$	−29
		.4 HCl in 5.6 $HClO_4$	30
		1.5 HCl in 4.5 $HClO_4$	9
Ce^{III}	Ce^{IV}	6 formal HNO_3	−42
		6 formal $HClO_4$	
		second order	−7
		first order	−26

Thus far we have said little concerning the importance of charge. An attempt to study this can be made by investigating the entropy of activation of the various reactions. Admittedly this approach must be taken with extreme caution, because the concentrations of the actual ionic species in solution will undoubtedly change with temperature and lead to an apparent entropy change if left uncorrected. Such a correction is extremely difficult, because the exact composition of the solution must be known, as well as the particular ionic species which are important for the oxidation and reduction. In principle, however, these can be determined. The entropies of activation of the various exchange reactions studied is given in TABLE 2.

The results are difficult to interpret. In the thallium system, the entropy changes from a negative to a positive number as chloride is added, in agreement with the hypothesis that negatively charged chloride complexes are formed. When one compares the cobalt, thallium, and cerium systems, however, no very marked trend is observed as the charge product is varied. One is thus led to the conclusion that, although the charge product is almost certainly of importance, other factors, such as spacial arrangements, preferred direction of approach of ions for reaction, and change in concentration of reacting species with temperature, are of equal or perhaps greater significance in determining the entropy of activation of these reactions.

Thus, we can summarize the factors that are most important in determining reaction rates in oxidation-reduction systems in the following manner.

(1) The nature of the species undergoing exchange:

(a) The presence of complexing agents which form new potentially oxidizable or reducible species with different spacial arrangements and free energies;

(b) the electronic state of the ions involved.

(2) Conditions that will cause different species to exchange at different rates:

(a) free energy change of the reaction involved;

(b) The charge product of the reacting ions;

(c) the electrical properties of the solution (determined chiefly by the ionic strength and dielectric constants).

It is unfortunate that no one of these factors appears to be of universal importance; hence, the *a priori* estimation of rates in a given case is at present impossible. We have seen, however, that in given cases one of the factors does appear to overshadow the others and that, by studying such systems, it is possible to gain some knowledge concerning the nature of ions in solution as well as the nature of the reactions involved.[*]

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SECTION OF PSYCHOLOGY

SEX DIFFERENCES IN PERCEPTION[*]

By H. A. WITKIN[†]

Although a great deal of information has been accumulated in recent years about differences between the sexes in regard to a variety of psychological functions, such as intelligence, aptitudes, motor ability, and so forth, little is known about sex differences in one extremely important function—perception. The reason for this is undoubtedly twofold. First, the theoretical approaches used in the study of perception have not been of a kind to encourage consideration of individual performance or individual differences in performance. Secondly, except in certain clinical situations, there has not existed any special need for evaluation or measurement of a given individual's perception, in the way that there has been a pressing need in many life situations for determination of a given person's intellectual level, special skills, or personality makeup. In the absence of a concern with the perception of individuals, it was not very likely that differences between people, or between men and women as groups, would be discovered.

The importance of knowing about sex differences in perception, or in any other psychological function for that matter, is not simply to extend the catalog of known sex differences. By establishing the way in which sex differences may affect a given perceptual function, it is possible to obtain a fuller understanding of the nature of that function. Further, since definition of the person as male or female is greatly affected by social factors, a study of sex differences provides an excellent approach to an understanding of the role of cultural influences in psychological development.

The Nature of Sex Differences in Perception

For some time, our laboratory has been engaged in a systematic investigation of the personal determinants of perception—that is, the general psychological characteristics of the individual (cognitive, motivational, emotional) that affect his perception. A variety of perceptual situations has been investigated and a large number of subjects, of both sexes, tested. In every situation studied, the performances of men and women were strikingly different. Furthermore, the differences were always of the same kind. To illustrate the nature of these differences, results of some of the tests will now be described.[*]

Orientation in Space. Using several different situations, a study was made of the way in which people perceive the position of objects, including their own bodies, in relation to the upright in space. Our ability to establish the upright as quickly, easily, and accurately as we ordinarily do has a twofold basis. First, the direction of gravity, which corresponds to the true upright, is readily apprehended through the continuous postural adjustments made to this force. Second, the visual environment has the character of a framework, whose main outlines represent the true vertical and horizontal; and this also provides a ready basis for establishing the upright. The cooperation between these two sets of factors is ordinarily very intimate, resulting in a unified impression of the upright. Furthermore, since the visual and gravitational uprights coincide in direction, the outcome is the same whichever standard is used as the main basis of perception. Several test situations were developed to investigate the manner and extent of the individual's dependence on one or the other of these standards. Two of these will be described, together with the results obtained for men and women.

One test evaluates the individual's perception of the position of his body and of the whole surrounding field in relation to the upright. The apparatus consists of a small room within which is a chair. Each may be tilted to left or right to any extent, either by the experimenter from outside the room or by the subject seated in the chair. They may be tilted alone or together, to the same side or opposite sides, and at the same speed or different speeds. The subject seated in the chair cannot see outside the experimental room, so his judgments of the upright must be based on whatever information he can obtain from his bodily experiences and from the visual field of the room. The test procedure consists of tilting the room and chair to set positions and then requiring the subject to adjust one or the other to the upright—the room on some trials, his chair on other trials. The positions to which room and chair are brought may be measured to the nearest degree, so that the results are expressed in quantitative terms.

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[*] This paper, illustrated with lantern slides, was presented at the meeting of the Section on October 17, 1949.

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[*] The material presented here has not yet been published.

In that part of the test in which the room had to be adjusted to the upright, most subjects tended to "go along with" the field in the sense that, to greater or lesser degrees, the perceived upright was shifted toward the vertical of the tilted room. In most cases, the perceived upright was located somewhere between the room's vertical and the vertical indicated by experiences of body position, with the result that the room was moved from its initial tilt to some position closer to the true vertical. In some extreme instances, the true upright was simply identified with the vertical of the room, and the room was perceived as fully erect at its initial position of 56° tilt. It was found that, in general, women accepted the room as straight at more extreme positions of tilt than did men, indicating that they are more apt to judge the upright in accordance with the position of the prevailing field. Thus, in one series of trials, where room and chair were initially tilted to the same side, men on the average perceived the room as straight when it was tilted 11.5°, women when it was tilted 17.7°. In another series of trials, with room and chair initially tilted to opposite sides, the room was perceived as straight by men at an average tilt of 22.9°, and by women at an average tilt of 30.3°.

In that part of the test where the subject was required to straighten his body with the room tilted, most subjects moved the chair in the direction of the tilt of the room, indicating that they based their judgments not only on how their bodies felt but also on how they looked—that is, whether or not they were aligned with the surrounding field. Women, on the average, tilted themselves farther in the direction of the field than did men. In one series of trials, where room and chair were initially tilted to the same side, men showed an average body tilt of 8.6° when they judged themselves to be upright, women an average body tilt of 12.5°. In another series, with room and chair initially to opposite sides, the average tilt for men was 7.8°, and for women 9.6°. Thus, women, in their perception of body position also, tended to be more strongly influenced by the surrounding field and to give less credit to bodily sensations than men.

The second test to be considered evaluates the individual's perception of the true orientation of an external item (other than his own body) surrounded by a restricted field. The apparatus consists of a luminous square frame, pivoted at its center so that it may be tilted to left or right, and a luminous rod, pivoted at the same center but moving independently of the frame. Since the test is conducted in a completely darkened room, all the subject can see are the frame and the rod. These are presented in tilted positions, and the subject is required to adjust the rod to the true upright while the frame remains in its initially tilted position. As a further variable, the subject's body is upright during some trials and tilted during others. It is, of course, much more difficult to make use of one's body in judging the position of a distant line when the body is tilted than when it is erect.

In this situation, it was found that, when asked to adjust the rod to the upright, most subjects displaced it from the upright in the direction of the tilt of the frame. Women, however, tended to align the rod with the tilted frame to a greater extent than did men, and this occurred both with body erect and with body tilted. With body erect, men tilted the rod an average of 7.4° from the upright when judging it to be straight, and women an average of 11.0°—almost invariably in the direction of the tilt of the frame. With body tilted to the same side as the frame, the rod was tilted 12.5° by men and 16.9° by women. Finally, with body tilted to the opposite side from the frame, the rod was tilted 14.5° by men and 19.1° by women. Under all conditions, women tended to adjust the rod in accordance with the position of the frame to a greater extent than did men, and they proved less able to involve the body in making their adjustments.

These findings, together with those obtained in other test situations, indicate that women, in judging the upright, tend to be more dependent than men on the surrounding field and to rely less on experiences from their own bodies.

Differences in Manner of Perception of Parts of a Complex Field. The orientation tests just considered showed sex differences in ability to perceive a part of a field (whether the subject's own body or an external object) independently of its surroundings. To determine whether these differences reflect a general tendency of men to be more analytical than women in their perception, a situation was devised which does not involve space orientation but which tests ability to isolate an item from its surroundings perceptually. In this test, the subject is required to find a simple figure within the pattern of a complex figure which is designed to obscure it as effectively as possible. To detect the imbedded figure, it is necessary to see it independently of its surroundings. The procedure used in the test is to show the subject, first the complex figure, then a copy of the simple figure that it contains, and, finally, the complex figure again, with instructions to locate the simple figure in it. Simple and complex figures are never shown together, and a new combination of simple and complex figures is used on each trial. The subject's score on each trial is the time taken to locate the simple figure within the complex figure.

It was found that men did better than women in this test, requiring less time to locate the simple figure. Whereas men took an average of 41.8 seconds per figure, women took an average of 57.8 seconds. This indicates that, for women, perception of the simple figure was more strongly affected by the structure of the field in which it was contained. Thus, the same kind of sex difference observed in the other two test situations appears here too. It is indeed striking to find a relation between the manner of perceiving the position of one's own body and the manner of perceiving a simple neutral visual design or a rod.

Differences in Manner of Maintaining Upright Posture and Balance. In the perception of body position, women were found to take greater account of the relation of the body to its surroundings and to give less credit to sensations from the body itself. To determine whether this reflected a general difference between men and women in manner of bodily orientation, sex differences in other tasks involving the body were investigated.*

One series of experiments evaluated the individual's ability to stand up straight while surrounded by visual fields of varying structure and stability. Under each condition of the visual field tested, the subject was instructed to stand up straight with feet together and hands at his sides. The amount of body sway was accurately measured by means of an ataxiameter device. When the test was conducted in a fully lighted room, men and women performed about equally well. The average amount of body sway per minute was 71.6 mm. for men and 70.2 mm. for women.† When the visual field was restricted to a simple luminous cube, which moved rhythmically from side to side, women showed a great deal more body sway than did men. Now, the average for men was 111.0 mm. of sway per minute, and that for women was 125.6 mm. With continued testing under this condition, however, women became adapted to the unstable field and, in time, were able to do considerably better. Thus, when confronted with unstable surroundings, women are initially made much more unsteady than men, although they are eventually able to come to terms with this instability.

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* This study was carried out with S. Wapner.

† These values are cumulative scores and are based on the sum of left-right and forward-backward movement in the head and hip regions.

A similar difference between the sexes was observed in another series of experiments, which evaluated the subject's ability to balance a platform on which he was standing while the field was varied in the same ways as described above. In this situation, women did more poorly than men, when tested in a fully lighted room, and became disproportionately worse when the moving luminous cube was used. Under the first condition, with a maximum score of 300 possible, men received a mean score of 66.7 and women a mean score of 55.9. Under the second condition, men received a mean score of 43.3 and women a mean score of 29.2. In addition, women showed marked reaction to the unstable field according to a second criterion of scoring; that is, they had to hold on a good deal more to a supporting rail in order to keep from falling over. Whereas men held on to the rails for an average of 7.2 seconds in each minute of testing, women held on for an average of 12.0 seconds. In this test, also, women showed some degree of adaptation with continued exposure to the moving cube.

Thus, as reflected both in their steadiness and in their balance, women are initially much more upset by an unstable environment than are men. This confirms the finding reported earlier regarding the difference in response to the visual field in perception of body position.

Differences in the Localization of Objects Providing Visual and Auditory Stimulation. Most of the situations described so far evaluated sex differences in relative dependence on bodily experiences and on the outer visual field in various types of activities. As an extension of this work, a situation was devised which juxtaposed visual and auditory, rather than visual and postural, experiences in order to determine whether the greater dependence on visual impressions that characterized the performance of women in our other situations would appear here, too.*

In this test, the subject was enclosed in a soundproof room, and the experimenter, standing outside at a glass window directly in his line of vision, read a story to him. The sound of the experimenter's voice was transmitted to the subject through two tubes, one to each ear. By varying the length of these tubes, it was possible to shift the apparent source of the sound to left or right of the center position where the experimenter was standing. Initially, the sound was made to come from this center, so that its visual and auditory locations coincided. As the subject listened, the sound was slowly shifted to one side, and the subject was required to indicate when it no longer seemed to come from the experimenter. If the subject detected a slight shift in the sound, he was, of course, locating its source on the basis of auditory impressions. If the sound moved a great distance before he perceived a change, he was locating it on the basis of visual impressions—that is, from where he *saw* it originate. As a control, the test was repeated with the subject's eyes closed.

It was found, first of all, that, with eyes closed, women and men did equally well in determining the direction of the source of the sound. For both, the sound had to be displaced an average of 5.3 cms. from center before it was perceived as coming from one side. With eyes open, however, women permitted the sound to be displaced much farther than men before perceiving that it had left the center position. In this case, the average values were 8.7 cm. for men and 11.1 cm. for women. Thus, under these conditions of auditory-visual conflict, women tended once again to rely more on visual impressions than did men.

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* This study was carried out with S. Wapner and T. Leventhal.

The results for all the tests just described indicate that, compared with men, women tend to be more affected by the nature of their surroundings and to rely less on impressions from their bodies. In the perception of a given field, also, women in general find it more difficult to extract an item from its surroundings and to perceive it independently of these surroundings.

An Investigation of the Origin of Sex Differences in Perception

The discovery that in all our tests there were striking differences in performance, not only between men and women as groups, but among individuals generally, and that, to an extent, an individual performed in a characteristic way in the different tests, raised the question of the origin of these differences. Since in each test the conditions of stimulation were identical for all, the problem became one of establishing the way in which personal factors, unique to the individual, determine his particular manner of dealing with given stimulus conditions. On the same basis, it appeared that the origin of the systematic differences in performance between men and women subjected to identical test conditions would be found in certain differences in personal characteristics between the sexes. Preliminary investigations indicated that the personal factors important in accounting for individual differences or for differences between men and women were at a personality level. Accordingly, an investigation has been undertaken* in which an intensive personality study is being made of each of a large group of subjects who are also receiving the battery of tests just described. Sufficient numbers of male and female subjects are being employed, so that, in addition to establishing the personality patterns associated with various modes of perception, it will be possible to determine the special personality characteristics of men and of women that make for systematic differences in perception between them.

In this work, which is still in progress (so that final results are not yet available), two kinds of population are being used: a normal group, selected from among students at Brooklyn College, and an abnormal group, consisting of patients at the Psychiatric Division of the Kings County Hospital.

As a further approach to the origin of sex differences in perception, and to individual differences generally, a study is being made of the perception of children of different ages. In addition to establishing perceptual "norms" for different age levels, this study is also investigating perception-personality relationships at each age. The objectives are: (1) to determine the bases of differences in perception at different ages; (2) to establish the origin of variations in performance within each age group; and (3) to determine whether the sex differences found among adults are present at all age levels.

These investigations should make clear the basis of the systematic differences in perception between men and women considered here.

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* This study is being carried out with P. Bretnall, M. Hertzman, K. Machover, and S. Wapner.

DIVISION OF MYCOLOGY[*]

SUSCEPTIBILITY FACTORS IN FUNGUS INFECTIONS IN MAN[†]

By STEPHEN ROTHMAN[‡]

The colonization of a dermatophyte fungus on the human skin and the course of the subsequent disease depend upon a great variety of susceptibility factors, the significance of which has been amply demonstrated clinically. These can be classified as external factors related to different properties of the skin surface and internal factors related to different internal situations, such as different nutritional and endocrine states, metabolic disturbances, and other systemic diseases. It happens, however, that internal factors act indirectly, by influencing the external milieu, or act only if, simultaneously, there are predispositional factors in the skin. Thus, it is not always easy to separate and evaluate the significance of internal and external factors.

A good example is the so-called "grand moniliasis," with infection by *Candida albicans* of all the large intertriginous areas, *viz.*, the axillae, submammary folds, groins and genitocrural folds, anal fold, and navel. Such a picture occurs most often in obese diabetic women. The appearance of the skin, for all practical purposes, is pathognomonic for diabetes. However, diabetes in itself is not enough to produce the picture. The patient must be a female and must be obese to develop this syndrome, and clinical experience indicates that much depends on the individually different degrees of moisture on the skin surface and on the tendency of the horny layer to be macerated by sweat.

This paper will deal with differences in susceptibility of different body regions, differences according to age and sex, pathologic internal factors, and individual susceptibility factors which at present cannot be classified otherwise because they are poorly understood.

Local Factors

Regional Susceptibility. Predilectional localization of dermatophytae in different body regions can be illustrated in infections with *Epidermophyton inguinale*, *Microsporon minutissimum*, *Microsporon furfur*, in infections of the feet with *Trichophyta*, and in *Candida albicans* infections.

That *E. inguinale* has a certain preference for the inguinal region is indicated in its name. The typical site of the lesion is the inner surface of the thigh close to the scrotal insertion, but quite frequently it is found also primarily in other folds, such as the axillae or the interspaces of toes. Certainly, it has a great preference for intertriginous areas, which is remarkable if we consider that it probably would have an equal chance to colonize outside the folds, mainly by transfer with the fingers. The most remarkable feature of *E. inguinale*, however, is not its great preference for folds, which is usually explained by the presence of moisture, but the fact that, after the fungus has thrived for a while in a favorable soil, it acquires the ability to spread outwards from the folds, for instance, to the buttocks. This event is rare, but it does occur; and one may speculate that the fungus, thriving for a while in a suitable medium, acquires new potentialities to live and multiply on a less favorable soil, in which it was unable to grow originally. The possibility of such acquired adaptation phenomena will be discussed later on.

Another fungus which is predilectionally localized in the genitocrural fold is *M. minutissimum*. Rarely it is found also in the axillae, but its occurrence anywhere else must be extremely rare. In the few cases reported with localization outside these folds, hot moist environment was regarded as cause for the unusual spread. That such an event is indeed exceptional becomes clear if we consider that erythrasma is "one of the most common skin diseases," whereas anomalous localization has been reported in only about a dozen of cases in the world literature.[1]

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[*] The Section of Anthropology held a meeting on Oct. 24, 1949, at which Dr. Frederick Richardson presented a paper entitled *General Results of Applied Anthropology*. An abstract of this paper will be published in a later issue of the TRANSACTIONS.

[†] This paper, illustrated with lantern slides, was presented at the meeting of the Division on October 28, 1949.

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In sharp contrast to *E. inguinale*, *M. minutissimum* does not grow out from the inguinal or axillary fold, although it is present in the folds for years or decades. This makes one think that possibly apocrine gland secretion products may make the soil favorable for *M. minutissimum* or, more than that, that such products possibly are obligate nutritional requirements. The horny layer in which the fungus lives imbibes the apocrine sweat. It is in the realm of experimental possibility to investigate which substances in apocrine secretions are essential. Such investigation would open the road towards a successful cultivation of the organism on artificial media.

Of course, tinea cruris caused by *E. inguinale* and erythrasma caused by *M. minutissimum* are biologically very different conditions. Tinea cruris is an active, spreading, infectious, inflammatory disease which can be cleared up with fungicides and may never return. Erythrasma is a quiescent saprophytism with no spreading tendency and with no, or hardly any, inflammatory response to the presence of the fungus. If *M. minutissimum* ever can be eradicated (I don't know whether it can be and certainly believe it unwise to try), it will recur again and will stay with the person, without growing and without disappearing, as long as he lives. Clinically, the difference between the two diseases is manifested by the presence of an active margin in tinea cruris, meaning more intense inflammatory signs on the edges than in the center, a feature which is conspicuously absent in erythrasma.

In contrast to fungi with a preference for intertriginous areas, *M. furfur* settles predilectionally in non-intertriginous parts of the trunk and the extremities. In spite of otherwise rapid spread, it likes to make a halt at the borders of such folds as the axillae or the submammary folds. So-called inverse localization of tinea versicolor, with exclusive involvement of the folds, is extremely rare.

Disregarding the contrasting localization and the difference in spreading tendency, the two conditions, erythrasma caused by *M. minutissimum* and tinea versicolor caused by *M. furfur*, have many features in common, making them vastly different from all other fungus diseases. They are caused by fungi which cannot be cultivated on artificial media; the fungi grow exclusively on the skin of certain individuals; they hardly cause any inflammatory reaction; they cannot be cured permanently (no matter how thorough and effective the fungicidal treatment is, they will recur); and, finally, they are not transferred from person to person, which implies that these fungus diseases are not contagious.

In both diseases, individual susceptibility must be assumed. There are persons whose skin surface has features which make it a good culture medium for *M. furfur* or *M. minutissimum.* The experience, that these conditions stay indefinitely or recur after treatment has been discontinued, induces us to assume that *M. furfur* and *M. minutissimum* are ubiquitous, or almost ubiquitous, organisms. They settle down and colonize on the skin of individuals who have a specific susceptibility, but they are unable to live and thrive anywhere else. Thus, the two conditions in question are infections in which the opportunity for contact is the least important feature, and susceptibility is of paramount significance.

The cause of such individual susceptibility probably has to be sought for in the skin itself. In the last two years, my laboratory has been engaged in the study of fat-soluble and water-soluble substances on the normal human skin surface. By applying two-dimensional paper chromatography in this study, we succeeded in isolating and definitely identifying eleven amino acids and in tentatively identifying five more.[2] These sixteen compounds are invariably present in water-wipings of the chest and back of normal adults. In carriers of tinea versicolor, some differences may be found in the composition of these amino acids or in the composition of fat-soluble materials whereby the skin surface will become an adequate medium for *M. furfur*.

The localization of tinea pedis ("athlete's foot") is rather stereotypical in fully developed cases, no matter whether it is caused by *T. mentagrophytes*, *T. purpureum*, or *E. inguinale*. Usually originating in the plantar fold of the 5th metatarsophalangeal joint, it involves the interspaces of the toes, the plantar surfaces of toes, the soles, and the sides of the feet not quite up to the ankles. True mycotic lesions containing live fungi never extend beyond this area. They are not found on the dorsa of toes and feet and the fungus does not creep up to the ankle or above it. This limited localizational area corresponds with those parts of the foot of which the skin does not contain hair follicles and sebaceous glands. A second peculiarity of tinea pedis is that, no matter how massive the infection and how long it has gone on without treatment, it hardly ever happens that the fungi are carried to distant parts of the body so that a tinea circinata would develop. A third peculiarity is that tinea pedis infection is cleared up much better and faster by application of the modern fatty-acid preparations than tinea circinata caused by the same fungus species anywhere else.

These observations lead to the following working hypothesis. The fungicidal-free fatty acids which are present on the normal human skin surface are products of sebaceous-gland function. In regions where there are no sebaceous glands the natural fungistatic power of the skin surface is weak or nonexistent. The localizational area of athlete's foot, therefore, is a rather easy prey for dermatophytae. In order that the same species may settle down in other regions, it must have acquired a certain degree of resistance to fatty acids, which seldom occurs.

The validity of this hypothesis can be proved if it is shown (1) that fungi are able to acquire resistance to fatty acids, (2) that regional differences in the fungicidal activity of the skin surface actually exist, and (3) that organisms deriving from lesions of the feet and from other parts of the body do display a different acid resistance.

Concerning the first point, Murphy and Rothman[3] were able to increase the resistance of *T. mentagrophytes* to pelargonic acid sevenfold by slowly and gradually increasing the concentration of pelargonic acid in the culture medium, *viz.*, from 0.002 per cent to 0.014 per cent.

The second point is difficult to prove, because of the deficiencies of the available fungistatic testing methods. With most methods, the main technical difficulty is to emulsify finely enough the rather large amounts of fat we have to use in the aqueous medium when the fungistatic action of surface fats is being tested. However, by applying, with some modification, the filter paper disc method of Kligman and Rosenzweig,[4] Dr. J. C. Murphy, in our laboratory, has obtained preliminary results which indicate that the fungistatic activity of ether-soluble material from palms and soles is significantly lower than that from other regions (TABLE 1).

TABLE 1

Inhibition of Fungus Growth by Skin-Surface Fats Obtained by Wiping the Skin of Different Body Regions with Ether-Soaked Cotton*

Region	*Zone of inhibition in mm.* *Average*	*Range*	*No. of Samples*
Scalp	20.02	13.5–24.0	5
Chest and back	14.45	10.9–23.8	5
Axillae	13.18	7.2–18.2	6
Palms	8.25	6.75–9.5	2
Soles	7.01	5.12–9.8	4

* Each sample was tested in at least two experiments. The filter-disc method proved to be reliable only if, on each individual plate, the inhibition zone of the test material was compared with that of pelargonic acid of known concentration.

So far we have been unable to prove our third point, namely, that fungi taken from foot lesions are more sensitive to fatty acids than fungi obtained from other parts of the body.

A highly characteristic regional susceptibility of the human skin is displayed in

Candida albicans infections. In otherwise normal persons, *C. albicans* has a strictly intertriginous localization on the skin. However, it is not restricted to folds containing apocrine glands, as is the case with *M. minutissimum*, but extends to all folds of the body surface, *viz.*, mouth corners, axillae, submammary folds, umbilicus groins, genitocrural folds, anal fold, and interdigital spaces of fingers and toes.

A most spectacularly stereotypical localization of monilia occurs in the web between the 3rd and 4th fingers. Not only is this "erosio interdigitalis blastomycetica" an extremely common disease, but also it was shown experimentally by F. Raubitschek[5] that it is easier to inoculate monilia artificially into this interspace than elsewhere. The erosio interdigitalis stays strictly in the interspace and does not spread to the dorsum of the hand, no matter how long the condition persists, whereas, if the same interspace becomes infected with a dermatophyton, the tinea will soon spread to the dorsum of the hand. The cause of the preference for the 3rd interspace is not known. It might be chemical or mechanical. Some dermatologists believe it is mechanical, due to the fact that this is the narrowest interspace and the skin folds are in closer approximation.

Role of Moisture. In addition to the regional susceptibility in erosio interdigitalis, as well as in all monilial intertrigos, there is another absolutely obligate susceptibility factor, namely, the inhibition of the horny layer with water. The interdigital erosion is a characteristic occupational disease of bartenders and housewives. It is hardly ever seen in people whose occupation does not necessitate continuous moistening of the hand. Continuous bathing in warm water, practiced by the old Viennese school in treatment of burns and pemphigus, all too often resulted in *C. albicans* infection ("water-bed moniliasis"). One hardly can escape the conclusion that *C. albicans*, like *M. furfur* and *M. minutissimum*, is an organism which is present everywhere, or almost everywhere, around man and that it is harmless for the skin as long as local or general susceptibility factors do not facilitate its turning into a pathogenic agent. *C. albicans* is often cultured from sputum and faeces of normal persons. Thus the opportunity of contamination is certainly greater than the incidence of cutaneous moniliasis would indicate. In his inoculation experiments, Raubitschek[5] was unable to maintain experimentally produced interdigital erosions for any length of time. They healed spontaneously in a few days, obviously because of the absence of predispositional factors.

General Factors

Sex. Susceptibility according to sex is most pronounced in erythrasma, with an overwhelming higher incidence in males. In a statistical study,[6] the male female ratio was found to be 39:2. This pronounced preference for one sex is in favor of the assumption that apocrine gland secretion plays a role in susceptibility to erythrasma, as it is known that this secretion is sex-specific. A similar, but not quite so strong, preference for males is shown by the *E. inguinale* infection of the groins. Tinea versicolor, which has no relation to apocrine glands, shows no sex preference.

Age. The best example to illustrate the influence of age on susceptibility to fungus infections is the spontaneous cure in puberty of the *M. Audouini* infection of the scalp hair and the immunity of the adult scalp to this infection. The mechanism of this phenomenon has been dealt with in several publications of our laboratory.[7] It was found that the cure in puberty is due to pubertal changes in the sebaceous glands. With oncoming puberty, these glands secrete a higher concentration or a greater amount of fungicidal free fatty acids, *viz.*, saturated, straight-chain, fatty acids from C_7 to C_{11}. Because of this pubertal change, the infection can no longer spread from hair to hair, as the pathways of spread, namely, the follicular canal and the horny layer, have become imbibed with a highly fungicidal material.

It is probably due to pubertal development of apocrine glands that erythrasma and *E. inguinale* infection of the groins appear only in postpubertal age. The rarity of tinea versicolor in children, on the other hand, remains unexplained. Tinea versicolor was observed to disappear in old age.[8]

Hormonal Factors. These have been largely covered in the discussion of sex and age factors. In addition, however, it should be mentioned that, in the remarkable experiments of Reiss,[9] it was demonstrated that *T. purpureum* infections are easier to maintain in male rabbits if they are castrated. The same author has also shown that methyltestosterone is a powerful inhibitor and that α-estradiol is a moderately effective inhibitor of *T. mentagrophytes* and *T. purpureum in vitro.*[10] He was able to cure *T. purpureum* infections in castrated male rabbits by local application of methyltestosterone in ointments.[11]

Metabolic anomalies. The susceptibility of diabetics to monilial intertrigo has already been mentioned. The common interpretation of this phenomenon is that the glucose content of the epidermis and horny layer is increased in diabetes. Indeed, we can easily promote the growth of *C. albicans* on our artificial media by increasing their carbohydrate content. By analyzing total skin, Urbach[12] found considerably increased glucose values in diabetes and also in nondiabetics with decreased glucose tolerance. However, the results of Urbach should be checked by modern methods and, most of all, analyses of isolated epidermis, the layer in which *C. albicans* thrives, should be made, before this interpretation can be regarded as conclusive. Earlier work by Marchionini and Ottenstein[13] seemed to indicate that sweat which saturates the horny layer has a higher glucose content in hyperglycemia than normally. In recent careful studies, Lobitz and Osterberg[14] could not confirm these findings.

From the clinical point of view, it seems to be important that severe generalized moniliasis, in which the susceptibility of the whole tegument is so great that regional preferences are no longer respected, is also connected with disturbances of carbohydrate metabolism. Two of our case records illustrate this point.

(1) A 45-year-old housewife was admitted to the hospital with severe generalized moniliasis and poor general condition. She expired six weeks after admission in a state of extreme emaciation. She had no diabetes but a markedly diabetic glucose tolerance curve. She also had pronounced glycosuria during the tolerance test. At autopsy, a pancreatic cancer was found. There can be little doubt that this neoplasm caused the disturbance of carbohydrate metabolism and, indirectly, also the fatally increased susceptibility to monilia. This case was reported elsewhere in detail.[15]

(2) A 48-year-old lady teacher was admitted with generalized moniliasis but in fairly good general condition. The urine was free of sugar and the fasting blood sugar was normal, but the glucose tolerance test showed slightly decreased tolerance with glycosuria during the test. This patient recovered after local fungicidal treatment was combined with antidiabetic measures.

Lately, it has been observed in our department that recalcitrant *T. purpureum* infections, in contrast with infections with *C. albicans*, are associated with increased glucose tolerance. There are in man rare cases of *T. purpureum* infection in which the process is widespread, the size and number of circinate lesions is unusually great, and, in addition, there are atypical skin manifestations with positive mycologic findings such as grouped itching papules, crusty lesions, and even lichenification. These cases show unusual resistance to routine local treatment. Even if they respond to some degree, new lesions break out in new sites with exasperating speed. Thus, an otherwise easily manageable short-term disease becomes an extremely chronic and quite serious intractable skin ailment.

In the last three years, we have observed three cases of this type and, to our great amazement, we found that all three patients had an abnormally flat glucose tolerance curve. This increased glucose tolerance could not have been due simply to delayed intestinal absorption, because the intravenous glucose tolerance in the two cases tested so far also indicated increased tolerance. This phenomenon is being studied at present, and no explanation can be offered as yet.

Lymphoblastomas. In the last few years, a remarkably frequent association of deep mycoses with lymphoblastomatous diseases has been noted. Ten per cent of all torulosis-meningitis cases reported have been associated with lymphoblastomas, chiefly with Hodgkin's disease.[16] Four cases of histoplasmosis combined with lymphoblastoma were reported by Cawley and Curtis[17]: 2 patients with Hodgkin's disease and two with lymphatic leukemia. One patient with histoplasmosis had mycosis fungoides and one patient with torulosis suffered from myelogenous leukemia.[17] Paracoccidiodes was reported in association with Hodgkin's disease by da Silva Lacaz.[18]

The nature of such association is not clear. In some cases, it seems that the lymphoblastoma has been the primary disease. If this should prove to be true, one will be justified in regarding lymphoblastomas as susceptibility factors.

Monilial granuloma.[19] One of the most remarkable but poorly understood individual susceptibilities to fungus infections is a rare form of *C. albicans* infection which begins in infancy or early childhood, often as simple oral thrush, and ultimately involves three sites: the oral mucosa, the skin of the scalp and face, and the finger nails. Pathologically, the disease is characterized by the formation of granulation tissue extending deep into the corium, similar to that seen in deep mycoses. There is a tremendous proliferation of the organism in the infected sites. The overwhelming number of filaments is so unusual for a yeast infection that even experienced mycologists looking at the potassium hydroxide preparations would doubt their being *C. albicans.* The condition is essentially intractable, and the prognosis for life seems to be poor. All patients observed, for a considerable length of time, have died in early youth. The data are insufficient to decide whether they died from internal moniliasis or from intercurrent disease. The carbohydrate metabolism was normal in the cases tested.

That this condition is due to a particularly low resistance in otherwise normal persons and not to a particularly virulent strain of *C. albicans* is borne out in the observation that, if the organism is transferred from monilial granuloma to a person with normal resistance, the normal superficial and easily treated type of intertriginous moniliasis results. Hauser and Rothman, who observed one case with multiple horn formation, were able to collect thirteen more cases from the literature.[19]

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CONFERENCES HELD

SECTION OF BIOLOGY

"THE CHEMOTHERAPY OF TUBERCULOSIS—THE EXPERIMENTAL APPROACH"

JUNE 24 AND 25, 1949

The Section of Biology held a Conference on "The Chemotherapy of Tuberculosis—The Experimental Approach." Doctor Geoffrey W. Rake, The Squibb Institute for Medical Research, New Brunswick, N. J., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, June 24

EXPERIMENTAL METHODS

Morning Session. Chairman, H. McL. Riggins, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"The Use of the Rabbit," by Max B. Lurie, The Henry Phipps Institute, Philadelphia, Penna.

"The Use of the Guinea Pig," by A. G. Karlson and W. H. Feldman, Mayo Foundation, Rochester, Minn.

"Experimental Tuberculosis in the Hamster," by E. W. Dennis, Sterling-Winthrop Research Institute, Rensselaer, N. Y.

"The Use of the Mouse," by G. P. Youmans, Northwestern University, Chicago, Ill.

"The Use of the Mouse," by Richard Donovick, The Squibb Institute for Medical Research, New Brunswick, N. J.

"A Method for Evaluating Anti-Tubercular Activity in Mice," by M. J. Baker, M. E. Schlosser, and H. J. White, American Cyanamid Company, Stamford, Conn.

"The Use of the Chick Embryo," by H. F. Lee; The Children's Hospital, Philadelphia, Penna.

"The Use of Avian Tuberculosis in the Chick," by M. Solotorovsky, H. J. Siegel, and W. H. Ott, Merck & Company, Inc., Rahway, N. J.

DRUGS OF SYNTHETIC ORIGIN

Afternoon Session. Chairman, J. E. Perkins, National Tuberculosis Association, New York, N. Y.

"Experimental Studies in Para-aminosalicylic acid (PAS)," by J. Lehmann, Gothenburg, Sweden.

"Evaluation of the Sulfones and Streptomycin in Experimental Tuberculosis," by M. I. Smith, E. L. Jackson, and H. Bauer, United States Public Health Service, Bethesda, Maryland.

"Pharmacological Studies on the Sulfones," by E. O. Titus and J. Bernstein, The Squibb Institute for Medical Research, New Brunswick, N. J.

"Experimental Evaluation of Synthetic Drugs," by F. A. French, Harold Brunn Research Institute, Mount Zion Hospital, San Francisco, Calif.

"Clinical Evaluation of Chemotherapeutic Drugs in Tuberculosis," by A. M. Walker and J. B. Barnwell, Veterans Administration, Washington, D. C.

Saturday, June 25

DRUGS OF NATURAL ORIGIN

Morning Session. Chairman, Selman A. Waksman, Rutgers University, New Brunswick, N. J.

"Quantitative Enumeration of Viable Tubercle Bacilli in Cultures and Infected Tissues," by F. Fenner, S. Martin, and C. Pierce, The Rockefeller Institute for Medical Research, New York, N. Y.

"The Streptomycins and Neomycin in Murine Tuberculosis," by Geoffrey W. Rake, The Squibb Institute for Medical Research, New Brunswick, N. J.

"Are Present Laboratory Methods Adequate for the Evaluation of Anti-Tuberculous Agents?" by W. L. Steenken, Jr., Trudeau Foundation, Trudeau, N. Y.

"New Antibiotics, Substances from Higher Plants, and Their Evaluation," by Gladys L. Hobby, Chas. Pfizer & Co., Brooklyn, N. Y.

"Clinical Evaluation of Drugs in Tuberculosis," by L. B. Hobson and Walsh McDermott, The New York Hospital, Cornell University Medical College, New York, N. Y.

SECTION OF BIOLOGY

"MECHANISM AND EVALUATION OF ANTISEPTICS"

OCTOBER 28 AND 29, 1949

The Section of Biology held a Conference on "Mechanism and Evaluation of Antiseptics." Doctor Herbert L. Davis, Ethicon Suture Laboratories, Johnson & Johnson, New Brunswick, N. J., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, October 28

ANTIBIOTICS

Afternoon Session. Chairman, W. W. Umbreit, Merck Institute for Therapeutic Research, Rahway, N. J.

"The Metabolic Action of Streptomycin," by W. W. Umbreit, Merck Institute for Therapeutic Research, Rahway, N. J.

"The Effect of Penicillin upon Protein Synthesis by Bacteria," by Rollin D. Hotchkiss, The Rockefeller Institute for Medical Research, New York, N. Y.

"The Colloidal Nature of Antibiotics," by E. A. Hauser, Massachusetts Institute of Technology, Cambridge, Mass.

"Newer Antibiotics—Factors Influencing Their Antimicrobial Activity," by Gladys L. Hobby, Chas. Pfizer & Company, Brooklyn, N. Y.

"Interference with Antibiotic Activity by Surface-Active Agents," by Eleanor A. Bliss, The Johns Hopkins University School of Medicine, Baltimore, Maryland.

"Studies on the Synergism between Bacitracin and Penicillin; Correlation of *In Vivo* with *In Vitro* Results," by Frank L. Meleney and Balbina A. Johnson, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"An Approach to a Correlation between *In Vivo* and *In Vitro* Activities of Anti-Tubercular Compounds," by Richard Donovick, Geoffrey W. Rake, and E. O. Titus, The Squibb Institute for Medical Research, New Brunswick, N. J.

"Evaluation of the Paper-Disk Assay Procedure and Its Correlation with Phenol Coefficients," by Robert R. Hoffman and Charles R. Phillips, Biological Department, Chemical Corps, Camp Detrick, Frederick, Maryland.

SURFACE-ACTIVE AGENTS

Evening Session. Chairman, C. A. Lawrence, University of Michigan, Ann Arbor, Mich.

"Mechanisms of Action and Neutralizing Agents for Surface-Active Materials upon Microorganisms," by C. A. Lawrence, Hygienic Laboratory, University of Michigan, Ann Arbor, Mich.

"The Meaning of Bacteriostasis, Bactericidal Effect, and Rate of Killing," by Philip B. Price, Department of Surgery, Salt Lake Hospital, Salt Lake City, Utah.

"The Interaction of Surface-Active Agents and Proteins," by Harold N. Glassman, Biological Department, Chemical Corps, Camp Detrick, Frederick, Maryland.

"Conductometric Studies of Bactericidal Mechanisms," by D. N. Eggenberger and H. J. Harwood, Armour and Company, Chicago, Ill.

"A New and Direct Approach to the Evaluation of the Germicidal Efficiency of Semi-Solid Pharmaceuticals," by M. J. Foter and L. L. Nisonger, The Wm. S. Merrell Co., Cincinnati, Ohio.

"The Role of Antagonists in the Evaluation of Germicides," by E. G. Klarmann, Lehn and Fink Products Corp., Bloomfield, N. J.

"Antiseptics under the Food, Drug and Cosmetics Act," by Glenn G. Slocum, Food and Drug Administration, Washington, D. C

"Present Trends in Methods of Testing Antiseptics," by G. F. Reddish, Lambert Pharmacal Co., St. Louis, Missouri.

Saturday, October 29

MISCELLANEOUS CHEMICALS

Morning Session. Chairman, E. H. Spaulding, Temple University School of Medicine, Philadelphia, Penna.

"Halogens and Their Mode of Action," by Henry C. Marks and Frede B. Strandskov, Wallace & Tiernan Co., Inc., Belleville, N. J.

"Mechanism of Action of Antiseptic Agents," by D. E. Green and W. E. Knox, Institute for Enzyme Research, University of Wisconsin, Madison, Wisconsin.

"Iodine as an Antiseptic," by Louis Gershenfeld and Bernard Witlin, Philadelphia College of Pharmacy and Science, Philadelphia, Penna.

"The Phenomena of Resistance," by Orville Wyss, The University of Texas, Austin, Texas.

"The Toxicity Index Method for the Evaluation of Germicides," by A. J. Salle, Department of Bacteriology, University of California, Los Angeles, Calif.

"Relationship of Concentration and Germicidal Efficiency of Ethyl Alcohol," by H. E. Morton, University of Pennsylvania School of Medicine, Philadelphia, Penna.

"Evaluation of Mercurials as Antiseptics," by F. B. Engley, Jr., Biological Department, Chemical Corps, Camp Detrick, Frederick, Maryland.

"Assay of Antiseptics at Different Times after Application to Human Skin," by H. M. Powell and C. G. Culbertson, The Lilly Research Laboratories, Indianapolis, Ind.

"Mercurials as Antiseptics," by J. H. Brewer, Hynson, Westcott & Dunning, Baltimore, Maryland.

NEW MEMBERS

A. Elected from May 13, 1949, to September 15, 1949

LIFE MEMBERSHIP

Campbell, Marguerite E., B.S., Library, Medicine. Librarian, Chicago Medical School, Chicago, Ill.

Fetter, Dorothy, Ph.D., Gastroenterology, Nutrition. Assistant Professor, Brooklyn College, Brooklyn, N. Y.

Hancock, Allan, D.B.A., Oceanography. Instructor, University of Southern California, Los Angeles, Calif.

Heilman, Fordyce Russell, M.D., Medical Bacteriology. Consulting Physician, Mayo Clinic, Rochester, Minn.

Hershberg, Emanuel B., PhD., Pharmaceutical Research. Chemist, Schering Corporation, Bloomfield, N. J.

Levy, Joseph, M.D., Metabolism, Cancer. Attending Physician, New Rochelle Hospital, New Rochelle, N. Y.

Malin, Edward W., Jr., M.A., Clinical Psychology. Psychologist, Veterans Administration, New York, N. Y.

Turell, Robert, M.D., Proctology. Adjunct Surgeon, Montefiore Hospital, New York, N. Y.

Zamanis, George H., B.Sc., Industrial Fermentations. Independent Consulting Chemist, Modesto, Calif.

SUBSTAINING MEMBERSHIP

Allen, Fred E., D.V.M., Diseases of Poultry, Mastitis of the Dairy Cow. Station Veterinarian and Associate Professor of Veterinary Medicine, University of New Hampshire, Durham, N. H.

Auger, Carlton, M.D., Pathology. Associate Professor, Pathology, Laval University, Quebec City, Canada.

Blumenfeld, Charles M., M.D., Pathology. Pathologist, Sutter General Hospital, Sacramento, Calif.

Chapman, S. Stephen, Ph.D., Biology. Director, Pharmacology Division, Schenley Laboratories, Inc., Lawrenceburg, Ind.

Dickie, Robert B., Public Relations. New York, N. Y.

Eggers, Carl, M.D., General Surgery. Consultant, Surgery, New York University, New York, N. Y.

Friedman, Milton, M.D., Radiation Therapy of Cancer. Assistant Professor, Clinical Radiology, New York University, New York, N. Y.

Goldhaft, Arthur D., V.M.D., Poultry Pathology. Director, Vineland Poultry Laboratories, Vineland, N. J.

Karush, Aaron, M.D., Pscyhoanalysis. Instructor of Psychiatry, Columbia University, New York, N. Y.

Kelly, Arthur L., D.V.M., Veterinary Medicine. Director of Research, San Diego Zoological Society, San Diego, Calif.

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Kimler, Alexander, Ph.D., Bacteriology. Bacteriologist, Jackson Memorial Hospital, Miami, Fla.

Lee, Clarence E., B.S., Poultry, Animal Nutrition. Vice-President, Director, The Beacon Milling Company, Inc., Cayuga, N. Y.

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Smith, Clarence A., Ph.D., Biochemistry, Nutrition. Technical Director, Agricultural Department, Standard Brands Inc., New York, N. Y.

Sowa, Frank J., Ph.D., Organic Research. Owner, Sowa Chemical Company, New York, N. Y.

Van Waveren, Erlo, B.A., Philosophy. Consultant, Independent, New York, N. Y.

ACTIVE MEMBERSHIP

Aaron, Raymond, General. Independent, New York, N. Y.

Aaron, Theodore H., M.D., Internal Medicine. Fellow, Medicine, Lahey Clinic, Boston, Mass.

Abbott, John N., M.D., Pathology. Senior Pathologist, New York State Department of Health, Albany, N. Y.

Adamson, Arthur W., Ph.D., Physical Chemistry. Assistant Professor of Chemistry, University of Southern California, Los Angeles, Calif.

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TRANSACTIONS
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SECTION OF PHYSICS AND CHEMISTRY

VISCOSITY OF POLYELECTROLYTES[*]

By RAYMOND M FUOSS[†]

Polyelectrolytes are electrolytes of high molecular weight which are derivable from a (sometimes hypothetical) simple monomeric unit. Proteins, polysaccharides, and nucleic acids are familiar examples of naturally occurring polyelectrolytes. (It might be mentioned that the definition is designed to exclude colloidal electrolytes such as oxide sols, in which the conducting unit is a massive charged particle of fixed geometry.) Sodium polyacrylate is a typical synthetic polyelectrolyte containing a polyanion; addition of alkyl halides to polyvinyl-pyridine gives a polyelectrolyte containing a polycation.

When the polyelectrolyte has a chain structure, as in the synthetic materials just mentioned, the configuration of the molecule in solution can be expected to change with concentration. If we consider a very dilute solution of sodium polyacrylate, for example, intramolecular electrostatic repulsion between the carboxyl groups of a given molecule will cause the flexible carbon chain of the polyanion to extend itself to give a rod-like molecule. The solution exhibits one marked contrast to a dilute solution of a simple electrolyte such as sodium chloride. In the latter, an exploring element of volume (of dimensions large compared to interionic distances but small compared to the total volume) contains, on an average, equal numbers of positive and negative ions, and the ionic concentration in it is, within small fluctuation terms, equal to the total number of ions present divided by the total volume. In a polyelectrolyte, an entirely different situation obtains. Due to the structure of the polyion, the charges in the chain can diffuse no further apart than is permitted by the maximum extension of the chain. Hence, at *all* stoichiometric concentrations, however low, violent fluctuations in charge density will be encountered by the exploring element of volume. In the vicinity of a polyion, the corresponding charge density will always be high, while in the regions of the solution between polyions, only counter ions will be found. The polyion thus behaves like a droplet of concentrated solution, regardless of the bulk concentration.

The effect of the above distribution on configuration can best be followed by considering what happens when the dilute solution is concentrated. Interionic distances between polyions decrease, and, due to the high charge density in each polyion, counter ions will be attracted to the chains by coulomb forces. The resulting structure will have a lower net charge than the hypothetical, completely dissociated, chain electrolyte, and, as a consequence of the partial neutralization of the internal charge by counter ions, the chain molecule will begin to curl up under the influence of thermal motion. In other words, as stoichiometric concentration is increased, the polyions will change from extended rod-like configurations to coiled configurations similar to those exhibited by ordinary polymers like polystyrene in solution.

These qualitative statements may be followed experimentally by means of viscosity measurements. Debye has proposed that the ratio of specific volume to concentration is a measure of the volume occupied by chain molecules in solution. For polyelectrolytes, we find that the reduced viscosity is very large (much larger than that of the uncharged parent polymer) in dilute solution, and that this value decreases as we go to higher concentrations. This result may be interpreted as hydrodynamic evidence for the curling of the polyions, as intramolecular repulsion is neutralized due to association of counter ions to the chain ions under the influence of electrostatic attraction. Data on a variety of aqueous and non-aqueous systems show that the phenomenon is general. Furthermore, systematic variations in the constants of the corresponding (empirical) viscosity function with the dielectric constant of the solvent support the theory that the viscosity changes are controlled by electrostatic forces.

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[*] This paper, illustrated with lantern slides, was presented at the meeting of the Section, November 1, 1949.

[†] Sterling Chemistry Laboratory, Yale University, New Haven, Connecticut. This is Project NR 054-002 of the Office of Naval Research.

By addition of simple electrolyte to polyelectrolyte, the theory may be tested further. We would expect that an excess of simple electrolyte would provide an adequate reservoir of counter ions to permit essentially complete neutralization of the charge on the polyions. As a consequence, the chains would contract, due to mutual attraction of ions of opposite charge in each polymeric molecule, much as a crystal of sodium chloride is stabilized by internal attraction. Experimentally, we find that the viscosity of a polyion in the presence of an excess of simple electrolyte is reduced far below that of the parent molecule. Moreover, the limit reached depends on the size of the counter ion and is independent of the ion of like charge. Data have been obtained in both aqueous and non-aqueous systems, which follow the pattern just described.

As a further example of the effects of coulomb forces on the hydrodynamics of polyelectrolytes, let us consider the effect of the addition of hydrogen chloride to a dilute solution of polyvinylpyridine in anhydrous methanol. The polymer is electrostatically neutral and behaves much like polystyrene, except that it is a weak base. If we add a little hydrogen chloride, some of the pyridine groups in the polymer are converted to pyridonium ions. Their mutual repulsion causes the coils to expand, and we observe a rapid increase in viscosity. As more hydrogen chloride is added, this increase continues, until we reach the saturation limit determined by the pK of the base. Subsequent addition of hydrogen chloride now decreases the viscosity, due to association of the excess counter ions with the chain ions, in just the same way as the addition of excess potassium bromide depresses the viscosity of polypyridonium bromide solutions. In other words, a plot of reduced viscosity against ratio of hydrogen chloride to nitrogen shows a maximum. The height and location of the maximum depends, naturally, on the total initial concentration of polymer.

As a final example of the strong electrostatic effects in polyelectrolytes, it has been observed that polycations precipitate polyanions at concentrations as low as ten-millionth normal (less than 0.1 ppm by weight). Both light-scattering work and gravimetric analyses show that these precipitates are not stoichiometric. Rather, they depend in a predictable way on the sequence of addition and on the relative concentration of the two polyelectrolytes concerned.

SECTION OF GEOLOGY AND MINERALOGY

THE GRANITE PROBLEM IN THE HUDSON HIGHLANDS*

By KURT E. LOWE†

The Granite Problem. It would be more exact to speak of problems in the plural, since many aspects of the formation of granites are still controversial. The fundamental issue is the origin of the granite magma or granite-forming fluid about which there is the least knowledge and the most conjecture. Other problems involve mode of emplacement, age relations, megascopic and microscopic structure of granites. The principal controversy, however, which has engaged geologists for more than half a century, deals with the formation of granites in the environment in which we now find them. Two opposing schools of thought have developed:

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, November 7, 1949.

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(1) The "magmatic" (traditional) school believes that granites are formed by intrusion of a true melt or magma of granitic composition into the rocks of the earth's crust.

(2) The "granitization" (relatively modern) school maintains that granites are the result of transformation of pre-existing rocks in place. Granitization, here used in its broadest sense, includes, according to Grout,[2] "a group of processes by which a solid rock (without enough liquidity at any time to make it mobile or rheomorphic) is made more like granite than it was before, in minerals or in texture and structure, or both." This may be accomplished by emanations from an igneous source penetrating the rocks of the solid crust either in the form of hot, aqueous solutions (hydrothermal) or as gases (pneumatolytic). Ionic transfer in the solid state has also been proposed as a granitization process, but seems to be as yet little more than a theoretical concept lacking reliable criteria for recognition in the field.

Most proponents of granitization favor hydrothermal solutions as "granitizing agents" involving the process of metasomatism, which was defined by Lindgren[3] as "an essentially simultaneous, molecular process of solution and deposition by which in the presence of a fluid phase, one mineral is changed to another of differing chemical composition."

It is the purpose of this paper to discuss the field evidence on granites obtained by the writer in the Hudson Highlands and to examine it in the light of the two schools of thought.

The Hudson Highlands. The Highlands are a chain of low, but rugged, mountain ranges extending about 140 miles from Reading, Pennsylvania, northeastward through northern New Jersey and southeastern New York into western Connecticut. They are mostly Precambrian crystallines, representing an ancient orogenic belt of Grenville(?) sediments which were folded, faulted, metamorphosed, and invaded by several igneous phases.

The Hudson Highlands, a rather loose geographical term, refers to that portion of the mountain chain which lies athwart the Hudson River in New York State. Berkey and Rice[1] mapped the West Point quadrangle and provided the first comprehensive treatment of Highlands geology on both sides of the Hudson River. The writer's studies have been confined to the region west of the Hudson River (Schunemunk quadrangle), with particular emphasis on detailed work in the Bear Mountain area at the southeastern gateway of the Hudson gorge.

Regional Structures. The folded structure of the Highlands is clearly indicated by the topography of the Bear Mountain region (FIGURE 1). Ridges of resistant Storm King granite, which is conformable with the structure of the country rocks, depart from their predominant northeasterly trend (Stockbridge and Long Mountains and Holmans Hill, or the Crown Ridge and Bare Rock). The trends of Turkey Hill, Summer Hill, West Mountain, The Timp, and Dunderberg (the last two not shown on map) describe a distinct arcuate pattern with Bear Mountain as the center. Field measurements proved the existence of a large, synclinal structure with a northeast plunge of 40° at Bear Mountain, steepening rapidly to 50° and 60° at Fort Montgomery.

The major, probably Precambrian, faults (FIGURE 1) strike northeast, *i.e.*, parallel to the tectonic trend, and indicate overthrusting to the northwest. The more northerly strike of the prominent Timp Pass-Hudson River fault (north of Doodletown Brook) suggests possibly Paleozoic origin (Taconic or Appalachian). Overthrusting along this fault is believed to account for the absence of the eastern limb of the Bear Mountain syncline and for intense crumpling, shearing and, overturning of minor folds in the Bear Mountain Inn-

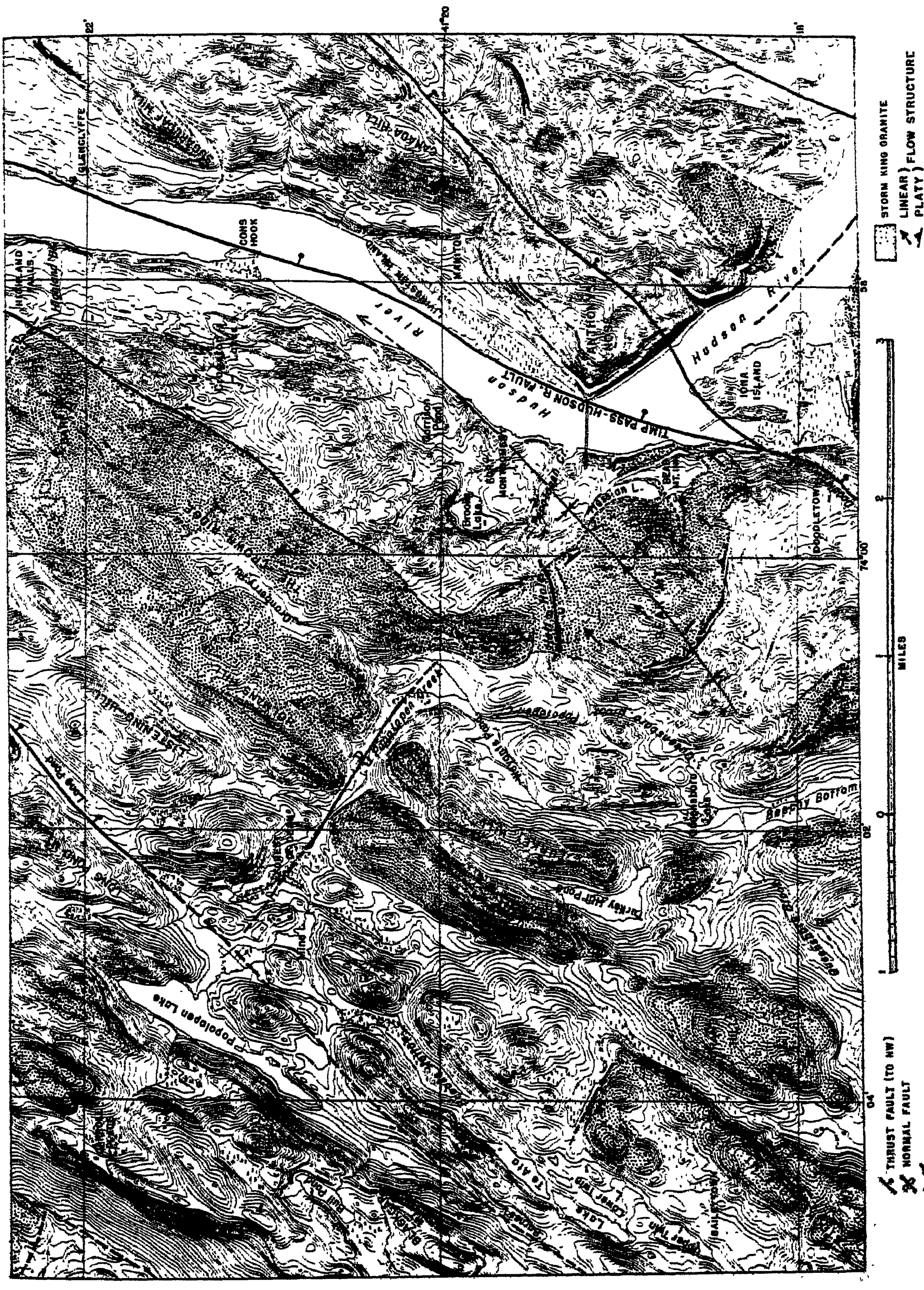

FIGURE 1. Hudson Highlands in the vicinity of Bear Mountain, N. Y.

Doodletown area (thrust sole). Cross faults at Highland Brook, Popolopen Creek, and Hell Hole are interpreted as high-angle tear faults with large vertical components of displacement.

Hudson Highlands Granites. Two major granite types can be distinguished. The older Canada Hill granite phase includes the Mahopac, Reservoir, and Canada Hill granites of Berkey and Rice.[1] This Precambrian granite is conformable with the Grenville(?) metasediments and contains quartz, albite-oligoclase, perthite, and variable quantities of biotite. Synkinematic origin is indicated by secondary foliation concordant with the structure of the folded metasediments and plastic deformation (boudinage) structures of the pegmatitic facies.

The younger Storm King granite is postkinematic and of uncertain age. Microcline-microperthite is the dominant feldspar and hornblende the dark mineral. Quartz content varies from 5 to 30 per cent by volume. It is entirely conformable with the structure of the Highlands Complex, a term used by the writer to include all Highlands crystallines of pre-Storm King age.

Canada Hill Granite Phase. The most typical representative is a medium-grained, medium-gray, biotite granite with faint to excellent foliation depending on the quantity of biotite present. East of the Hudson River, it occurs in large masses (Anthonys Nose and Canada Hill) and was interpreted by Berkey and Rice[1] as the result of intrusion of a "highly reactive and insidious magma of bathylithic proportions." The authors, however, also mentioned the effects of "mineralizers and emanations in a very fluid condition at high temperature and pressure." Thus, both magmatic emplacement and metasomatism were believed to have been responsible for the formation of this granite.

The Canada Hill granite phase at Bear Mountain appears to be represented only by granitic layers intercalated with the metasediments (biotite, hornblende, and augite gneisses; biotite, graphite, and epidote schists; and graphitic marbles). Several such layers are exposed along the Perkins Memorial Drive (southwest flank of Bear Mountain; below Storm King granite contact) and can be traced some 600 feet along the strike. They have quite constant thickness and their contacts with the intervening dark gneisses and schists are sharp. Some are remarkably uniform, medium-grained, quartz-feldspar rocks with discontinuous pegmatitic stringers oriented parallel to the layered structure. The writer has called these rocks "pseudoalaskites," since their intrusive origin is questionable. Microscopic features, such as intimate intergrowth of highly poikilitic feldspars, and extensive replacement of quartz by feldspars seem to be the result of metasomatism and recrystallization.

Other layers contain sufficient biotite to make the rocks indistinguishable from the typical Canada Hill granite of Berkey and Rice[1] in hand specimen and under the microscope. Also, there are no dikes or offshoots from these granitic layers into the adjoining fissile metamorphics, as one might expect had these rocks been introduced as a magmatic melt.

The evidence then points to some process of selective reworking of sediments (possibly rather pure arkosic sandstones) in place by hydrothermal solutions, rich in alkalies.

Storm King Granite. This youngest granite of the Hudson Highlands has remarkably uniform structure and composition, which make it the most distinct lithologic unit in this terrane of complex rocks. The typical Storm King granite is a medium- to coarse-grained rock, dull gray on fresh exposures, but sometimes with a greenish to pinkish tinge and somewhat greasy luster. Exfoliation is prominent, and the residual mantle has a "rubbly" character.

Linear parallelism of prismatic hornblende crystals and crystal aggregates is characteristic and gives the rock a streaky, gneissoid appearance on most exposures. This linear structure has a constant orientation of N.40°E.40°, or nearly parallel to the tectonic axis of the Bear Mountain syncline (FIGURE 1). In the marginal portions of the granite mass, the linear hornblende elements commonly acquire an additional plane-parallel alignment. Such platy or planar structure has invariably the same attitude as the nearest granite contact surface (FIGURE 1).

At Bear Mountain, this granite occupies the core of the syncline (maximum thickness: 3000 feet) and extends in sheetlike fashion along its western limb, forming the continuous ridge crests of The Torne, Crown Ridge, and Bare Rock (FIGURE 1). Contact exposures aggregating some 5000 feet prove that the granite mass is entirely concordant with the structure of the Highlands Complex. Contacts are sharp and show no gradational transition. Xenoliths of the country rocks are found only in the near-contact portions of the granite. They generally maintain the same structural attitude as the adjacent parent rocks and exhibit sharp, angular borders without any evidence of fusing.

Primary joints are filled with aplite, quartz, and epidote and coated with chlorite and sericite. The coarse-textured pegmatite facies of the Storm King granite is characteristically pink and quartz-rich and contains large, unoriented hornblende crystals. Several such pegmatite bodies within the granite mass have indistinct, gradational margins and centers of graphic intergrowth between potash feldspars and quartz. Storm King pegmatite dikes are not uncommon in the Highlands Complex, but only in one instance could a dike be observed cutting across the granite contact.

Certain distinct mineralogical changes occur in the granite near contacts with the Highlands Complex. Composition of this contact facies seems to be related to the lithology of the adjacent country rocks. Increasing quantities of biotite, graphite, garnet, and plagioclase feldspars are usually present near contacts with basic Grenville (?) gneisses and schists. Where Storm King granite is next to granitized metasediments (Canada Hill phase), its contact facies seems to be characterized by lack of dark constituents, increase in potash-soda feldspars (perthite), and abundance of quartz.

Petrofabric study revealed that quartz grains in the interior of the Storm King granite mass have no clearly preferred space-lattice orientation. There is certainly no evidence of a tectonic pattern of orientation of the quartz *c* axes as stipulated by Sander[7] for fusion tectonites.

Emplacement of Storm King Granite. The evidence cited appears to be in favor of magmatic intrusion of Storm King granite in the form of a synclinal pluton following the pre-existing structure of the Highlands Complex. Linear and platy mineral structures are interpreted as due to magmatic flow which caused parallel orientation of early hornblende crystals in the viscous melt. Rather uniform lithology, sharp contacts, shape, and position of xenoliths serve as corroborating evidence. Magmatic segregation would account for the gradational pegmatite bodies within the intrusion, and the contact facies can be explained by reaction of the magma with its wall rocks. The most probable source of the primary joint fillings was hydrothermal end-stage activity of the magma.

Postkinematic nature of the intrusion is indicated by complete absence of secondary foliation and lack of preferred (tectonic) orientation of quartz space-lattices. Since this sizable granite pluton was evidently the result of quiet intrusion at depth and under no great deformational stress, it is unlikely that it made room for itself by lifting the overlying country rocks. It is suggested, therefore, that gradual sagging of the synclinal structure under plastic conditions into the emptying magmatic chamber below created space in the more solid rocks above, which in turn was occupied by the rising magma. This concept of "exchange of space" was first proposed by Loewinson-Lessing[4] to explain the emplacement of large, gently dipping trap sheets in Siberia.

Age Correlation and Origin of the Highlands Granites. In the Hudson Highlands, the

Storm King granite is unquestionably younger than the Canada Hill granite phase, which is believed to be Precambrian. In the absence of radioactive age determinations, the Storm King granite could be Paleozoic as far as the available record goes. About 12 miles southwest of Bear Mountain, this granite disappears as a recognizable intrusive unit, and mixed granite gneisses take its place, suggesting formation by granitization rather than magmatic intrusion. The Byram and Losee granite gneisses of New Jersey and Pennsylvania, which were correlated by Berkey and Rice[1] with the Storm King and Canada Hill granites, respectively, appear to be of nearly the same age, because they have been found cross-cutting each other.

This age discrepancy and somewhat differing mode of formation of the principal granitic phases in these two contiguous sections of the Highlands may be simply a function of the regional structure. The continuous, tectonic structure of the Highlands has a consistent northeast plunge. Thus, the crystalline rocks of the Hudson Highlands represent a younger stratigraphic horizon than those of New Jersey and Pennsylvania. Both granites probably originated from a single magmatic source, in view of their very similar chemical composition, which differs to any extent only in the potash-soda ratio. In the southwestern Highlands, granites were formed at the greatest depth, *i.e.*, under greatest pressure and temperature. In such environment the magmatic fluid was probably very liquid, mobile, and chemically active, since it still contained all its volatile constituents, and therefore was particularly well suited to act as "granitizing agent."

The most volatile and reactive components (soda-rich in this case) then penetrated the upper rock horizons, causing formation of the Canada Hill granite phase by metasomatism. At the same time, the viscosity and chemical stability of the residual liquid gradually increased, with loss of the volatiles, until a true magmatic melt had formed. With increasing viscosity, this (potash-rich) magma intruded ever more slowly into the higher levels of the crust to form the Storm King granite.

In conclusion, it might be suggested that the opposing camps of "granite makers" are not nearly as far apart as they would have us believe. It is quite plausible that granitization and magmatic intrusion are merely phases of one and the same process and depend largely upon the crustal horizon at which the formation of a granite takes place.*

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* A detailed and documented treatment of the subject matter discussed in this paper will appear under the title "Storm King Granite at Bear Mountain, New York" in the Bulletin of the Geological Society of America, February or March, 1950.

SECTION OF BIOLOGY

EFFECT OF IONIZING RADIATIONS OF GAMETES, EMBRYOS, AND THE NEWBORN*

By ROBERTS RUGH†

Since we are all breathing radon and thoron into our bodies, and since neutrons from cosmic rays are transmuting nitrogen into C^{14} in our bodies at all times, we now admit that we must contend with ionizing radiations as we do with death and taxes. It is true that the total cosmic and gamma radiations to which the average man is subject may be less than 0.3 mr. per day or 0.1 r. per year. This is enough to account for the ionization of about 1 molecule in one hundred billion molecules per year. This doesn't seem like very much, and, biologically speaking, it may never prove to be much. It must be pointed out, however, that 1,000 r. of X irradiation will ionize only about 1 molecule in ten million, and yet this is sufficient to be lethal for most organisms. Biologically speaking, there may be some 10^{10} molecules in the average cell, but only 0.00001 per cent of them need be ionized to cause the death of most cells. From the point of view of the living organism, therefore, we must admit that the effect of ionizing radiations must be better understood.

Ionizing radiations may be divided into two major categories, again speaking biologically. Those which affect the organism from without, or from external sources, are:

Alpha Particles. From polonium, radon, or the bombardment of boron or lithium. The ion density of alpha emitters is very great, but penetrance is very low. That is, the number of ions per cubic millimeter is relatively very great, but no alpha particle can penetrate biological tissues to any great depth; for instance, they cannot get through the outer dead layer of skin.

Beta Rays. Pure sources would be from isotopes of Phosphorus (P^{32}), Sulphur (S^{35}), Carbon (C^{14}), and others. The penetrance of beta rays into tissues is somewhat greater than alpha penetrance, but, at 1 mm. depth, about 60 per cent of beta rays are absorbed by the skin when derived from phosphorus. There are mixed sources of beta rays, combined with gammas, as in sodium, potassium, and iodine.

X Rays. Depending upon whether the X rays are low voltage (soft) or high voltage (hard), there will be different degrees of penetration of any body of tissue. This fact is being used to treat superficial or deep tumors with X rays.

Gamma Rays. These may be obtained from some isotopes, by filtering out the betas, from Radium A, and from other sources. The ion clusters from gamma irradiation are somewhat farther apart than the clusters for even the hard X rays.

Neutrons. These are derived from the pile. The "n" equivalent of the "r" has not been satisfactorily determined against biological material.

The internal sources of ionizing radiations are the various isotopes which may be implanted, injected, or ingested. The broad and exciting field of research and therapy opened up by the discovery of isotopes is not to be discussed here, but it might be pointed out that, from the standpoint of control of irradiation of specific regions (with the exception, possibly, of iodine and metastatic thyroid carcinoma), the isotopes are not yet as easily controlled as the external sources of irradiation. This is due in part to the facts that decay, excretion, and diffusion throughout the body may be somewhat difficult to control and that there are great variations in the type and the energy of particles emitted from the different available isotopes.

Mention should be made of three of the six major theories concerning the biological action of ionizing radiations, not because any one of them is considered satisfactory but rather to point up the fact that we are a long way from understanding just what happens when such ionizations are brought about within the cell.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, November 14, 1949.

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(1) *Enzyme Inactivation Theory.* Recognizing the powerful influence of enzymes in normal metabolism and the fact that so few molecules of a cell need be ionized to knock it out completely, one is tempted to attribute the damaging effects to the inactivation of these vitally important protein enzymes. This theory may be tied in with the fact that the water molecule is so abundant, that it is therefore probably the most frequently ionized molecule, and that it is functional in enzyme activity. This theory of enzyme inactivation has as its supporters such men as Barron, Dale, Rudisill, and Hoch.

(2) *Fluid Flow Theory.* This is the theory of Failla, which states that nuclear and cytoplasmic swelling, which follow irradiation, is due to excessive ionization of molecules peculiar to the cell so that there is an excessive endosmosis. This is plausible as long as we admit that a semipermeable membrane intervenes—a membrane which will not allow egress of many of the newly ionized particles.

(3) *Point Hit Theory.* This is also known as the "target theory" of Lacassagne, Glocker, Lea, Crowther, and others. It implies that within the cell there exist specific sensitive volumes within which ionizations are biologically effective. Any ionization, to be effective, must hit the target volume. Other ionizations could occur within the cell, but they would be ineffectual since they would then concern insensitive volumes.

The first two theories mentioned incorporate the end result within the theory (enzyme inactivation and fluid flow), while the latter tries to explain those end results by postulating differential sensitivities within the cell for which there is some circumstantial evidence. None of these, or other theories, however, as yet explains why the ionization of 1 molecule in 10 million will kill a cell or organism.

[Lantern slides reporting investigations on a wide variety of forms, from amphibia and birds to mice and hamsters, were shown.]

Some of the conclusions which might be drawn from these and other works, are given in the summary which follows.

(1) Ionizations, to be biologically effective, must be absorbed by the living system. Mere exposure is no guarantee of absorption. At high levels, the intensity of irradiation is not as important as is the total dose, but, at low levels, fractionation seems to allow interradiation recovery.

(2) There is insufficient evidence in support of the Arndt-Schulze Law, which states that small doses accelerate, medium doses depress, and large doses destroy.

(3) There seems to be no differential susceptibility of the sexes, apart from their gonads. In most cases (Amphibia seem to be an exception), the ova are more sensitive than the sperm.

(4) Heavier animals, of the same age and species, seem to be the more resistant to irradiation damage.

(5) Multimodal factors appear in death of many forms, suggesting that the mechanisms which bring about the death of an organism may be multiple.

(6) There is always a latent period between irradiation and evidence of the biological aftereffect. The shortest interval of demonstrable effect seems to be four hours, and the longest may be a matter of weeks. Cell damage may occur without any evidence until a subsequent attempt at mitosis is made.

(7) Viruses, bacteria, eggs, and unicellular Protozoa, when irradiated, are affected very differently from a compact multicellular organism, largely because of the reduction in scattering and the lack of accumulated effects.

(8) Even within the lifetime of a single organism (*e.g., Drosophila*) there may be a 500-fold or greater difference in sensitivity to ionizing radiations, depending (at least in part) on the rate of cell proliferation at the time of exposure.

(9) Many environmental factors have been studied for their effect in protecting the organism or enhancing the radiation effect. Some of these are dryness (mois-

ture), oxygen (anerobiosis), temperature, anesthesia, nutrition, *etc.* Factors within the organism such as age, weight, metabolic activity, and rate of germination may all alter the reaction to ionizing radiations.

(10) The "Hertwig" effect (activation without syngamy by X-rayed spermatozoa), which has been demonstrated for the frog and for *Habrobracon*, has not been demonstrated for many higher forms. This may be because defective embryos are generally resorbed during intra-uterine life. In support of this contention is the case of Parkes,[1] in which rabbit eggs inseminated with sperm previously exposed to 25,000 r. X rays proceeded to cleave, though abnormally. As long as sperm are motile, they seem to be able to fertilize eggs, and frog sperm exposed to 120,000 r. X rays will activate normal eggs. The reduction in litter size, when the male mammal is X rayed, is probably due to resorption of damaged embryos.

(11) X radiation of gametes, either sperm or eggs at low levels, has no effect on the rate or pattern of the early cleavages. Irradiation of the zygote is far more damaging than comparable irradiation of either the gamete or the adult. X irradiation of *Arbacia* eggs, prior to fertilization with normal sperm, showed delay in cleavage to an increasing degree as the dose was increased from 10,000 r. to 100,000 r. The cleavage delay at controlled temperatures of 15°, 20°, and 27° C. can be used to determine the biological equivalent of roentgens when using types of irradiation other than X rays.*

(12) In the adult, the nervous system seems to be one of those most resistant to irradiation damage, while in the embryo this system is the most sensitive. Inhibition of growth, proliferation and desquamation of neuroblasts into the neurocoele, and interference with mitosis are some of the demonstrable effects of X radiation damage on the embryo. It is believed that survival is enhanced as long as the circulatory system remains undamaged. Sensitivity to radiations is reduced as the differentiation of organ systems is achieved, but the newborn seem to be more resistant than the aged.

(13) There is evidence that the isolated (egg) nucleus is less sensitive to X irradiation than when it is surrounded by yolk and cytoplasm.[2] In general, it is believed that the more damaging effects of ionizing radiations are on the nucleus and on its hereditarily influential genes. Cytoplasmic influences are not denied.

Much is known about the chemical effects of ionizing radiations, more is known about their physical effects, but relatively little is known about the biological effects with any certainty except that high exposures are fatal to all living matter. It is the province of the radiobiologist, in close and essential collaboration with the radiochemist and radiophysicist, ultimately to discover exactly why the ionization of one molecule in ten million is enough to interfere with the normal life processes so that death is inevitable in most cells and organisms.

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* It has thus far been impossible to develop a roentgen equivalent biological as the roentgen equivalent physical r.e p.) because (*a*) the whole organism responds in a manner different from its organ parts, (*b* the organism from the fertilized egg to senescence is not uniformly radio-sensitive, and (*c*) the various tissues, at any stage, are differentially sensitive.

SECTION OF PSYCHOLOGY

THUS SPEAKS THE BODY—AN ANALYSIS OF POSTURAL BEHAVIOR*

By FELIX DEUTSCH

Psychosomatic medicine investigates the sources of and motivations for the correlations and interrelations of emotional and bodily behavior. The conclusions from and interpretations of these integrated manifestations are, by and large, the fundamental basis of psychosomatic medicine. They represent the systematized knowledge of "how speaks the body." The study of motor behavior can contribute a great deal to the understanding of the personality from the psychosomatic point of view. For the past 5 years, I have been occupied with posturology, *i.e.*, the study of the unconscious motivations of postural behavior as it can be observed during the psychoanalytic sessions.

Three years ago, in a paper on "The Analysis of Postural Behavior," I expressed the opinion that close observation of the postural patterns of a patient can furnish the analyst with a readily available diagnostic tool, which may give him some clues for the existing psychodynamic process. I suggested that the more acquainted he may be with the basic postural patterns of his patient, the more apparent, significant, and indicative, deviations from this pattern will become to him. I emphasized, however, that the description of postural behavior and the analysis of some components of this behavior, which I had reported, were only a beginning of an attempt to introduce an additional clue to the understanding of unconscious motivation in the analytic settings. At the time of my last presentation, my observations were based on the analysis of 17 persons. In the following two years, 11 more cases could be added. Thus, at that time, I had 28 cases on whom those observations could be made. The procedure followed was to register any postures or changes of postures during every analytic hour and, likewise, the concomitant verbalized material.

It need not be emphasized that only the movements of the head, arms, and legs of the person lying on the couch, apart from the body posture as a whole, had been observed, since the analyst was sitting behind the person. The different postures of every hour were computed in a posturogram, on which the existing positions of the head, of the left and right limbs, and of the body were recorded.

From these posturograms the conclusions which follow can be drawn.

The configurations of the postures are bound to be characteristic for the individual in question. Their appearance will depend on: (1) the firmness of the synthesization of the postural pattern within the personality; (2) whether, during the free associative process, the ego control will be abandoned to such a degree that involuntary movements can break through; (3) the age period to which the individual regresses at a certain moment and the degree of erotogenization and symbolization of the part of the body in question, the number of inhibitory and defensive factors involved in the basic posture which governs the protective postural attitudes, or the need for the expression of the situation and emotional condition through the muscle apparatus of a certain part of the body; (4) the equilibrium between the instinctual drives and inhibitors and the pressure of conflictual instinctual drives which compete for their gratification. It can be stated with great certainty that the posture will change only whenever a shift in the psychologic situation takes place for which another postural expression is needed. The postural behavior substitutes, or sometimes precedes, the verbalization of the conscious material and is the forerunner of the appearance of the repressed unconscious. I assume that the signal to move very likely comes from subliminal sensations in the bodily sphere, particularly in the skin.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, November 21, 1949.

On one hand, the movements are always in relation to imaginative objects, or, on the other hand, somebody can become the object himself in relation to himself. The number and kind of emerging deviations from the basic posture depend on the existing emotional situation, and they change with the change of the situation. The emerging new postures are all predetermined and are released through the emerging unconscious material

The progress of analysis is very often expressed through the emergence of latent, expressive movements which are connected, by necessity, with verbalized material. These movements substitute preconsciously determined material and are the precursory expressions of an emotional content which was not yet consciously matured. Emerging postures seem to be the product of a long developmental process which very likely has its roots in the earliest childhood, depending on the psychologic development. Some of these postures betray their early origin, many showing an infantile character.

Much can be learned from the postural behavior of the hands and fingers. The postures of the hand seem to be less dependent on and interrelated with other parts of the body. During the analytic hour, three main postural behaviors of the hands can be observed: (1) they can be cupped, the right over the left hand or the left over the right; (2) they can be held in a flat position, not touching each other, more or less close together; or (3) they can be clasped, holding the fingers interlocked or interlaced. The thumb has a specific posture, *e.g.*, either it is covered by the fist or one thumb is cupped over the other or touches the other only with the uppermost joint.

This kind of detailed observation of the hand posture has brought the following conclusion: in accordance with the basic postural behavior of other limbs, there exists a basic hand posture characteristic for every person, *e.g.*, one person might always hold the right hand on top of the left, integrated with certain leg postures. Very often the change of the hand posture is accompanied by a change of the leg. Schilder has already pointed out that a great part of the body is discovered by the hand and that the hand itself is an outside world for those parts of the body which it touches and, vice versa, the body is an outside world for the hands. Parts of the body which can be easily reached by the hand have, therefore, a different psychologic meaning from those parts of the body which can only be reached by the hands with difficulty.

Some time ago, I saw a four-year-old, prematurely born, blind boy who could not talk and could even stand only with great difficulty. He made rhythmic pounding movements of the left hands against the bed, as well as against his own head. It was interesting that, in spells of completely inarticulated outcries, he suddenly turned in a protective attitude with his right hand towards the right ear while he tried with the left hand to attack and tear this ear. It seemed as if the right hand had taken over the protective, defensive attitude, whereas the left hand played the destructive, aggressive role. The boy, who had been frustrated in his relationships towards the outer world and had been deprived of the love of his mother, who rejected him, used his hands on his own body to act out his ambivalent feelings, assigning the positive feelings to the right hand and the negative feelings to the left hand.

An occasional observation of a catatonic patient showed that the arrested movements of his arms and hands were due to a tabooed relationship between certain symbolized and personified parts of the body, which he kept in resistive immovableness to each other. It could be shown, however, that the blockage of movement in his arms or hands was not equally inflicted upon other limbs. Those parts which had less genital representation and less identification with ambivalent objects would be freed more easily from the prohibited interrelationship and given freedom of movement. These arrested movements of different parts of the body suggest that the ego is afraid of the loss of integration, *i.e.*, of separating certain parts of the body from each other, of moving them away from a certain part of the body which has become sexualized or has achieved the role of an external love object.

Since the postural behavior is overdetermined, like most of the psychosomatic symptoms, arrested movement can mean, on one hand, the prohibition of giving in to a sexual impulse, or, on the other hand, the fear of acting out an agressive act. It may also mean a protective attitude against the hostile approach from outside, as well as the exposure to this approach. The flexibility of postural attitude depends on the personification and representation of certain parts of the body and their relationship to each other. Instinctual needs invested in them, with their drives for gratification and with their defensive and protective restraint, determine the attitudinal interrelationship.

It is impressive that, very often, the appearance of preconscious oral tendencies might be revealed almost regularly by a move of one hand to the mouth, either to cover the mouth or to hold the hand suckingly against the lips. During the revival of castration fears, the hands might selectively turn to the forehead, to the nose, to the neck, to the chin, or to the ear, as if in need of protection or as the target of aggression. This selective behavior is by no means a chance reaction and indicates with certainty, when it occurs, the underlying psychodynamic process. It is striking how selectively those hand-head relationships are predetermined and how disguisedly they force their expression.

A patient with severe obsessional neurosis assumed a strange hand-posture throughout her three year of analysis, building the prettiest architectonic edifices with her fingers. Eventually she became conscious that these fingers represented family members—two sisters, a brother, father, and mother—whom she made move as in a puppet show. The aggressive, destructive, and hostile feelings, as well as the friendly positive ones, toward them were acted out in continually changing positions of the fingers of both hands to each other which, according to their personification, played the role given them in the unconscious.

Another example of such predominant and selected use of fingers for the expression of dormant and unconscious strivings is the behavior pattern of a young man with female identification and latent, passive, homosexual tendencies, who always revealed his seductive, feminine phantasies by putting the fingers of his hand unconsciously in a triangular position illustrating the formation of the female genital or anal orifice. From this configuration, he changed back and forth to an interlaced and intertwined finger position, well known in passive dependent persons as the expression of holding symbiotically, so to speak, the feminine and masculine parts together. During a two-and-a-half-year analysis, the feminine hand posture of this patient was interrupted only twice for some time when, in rebellion against the paternal figure and later the brother figure, he reinstituted some of his masculine trends.

A female patient, with strong masculine tendencies, expressed in the hand posture—the right hand on top of the left—the unconscious phantasy of being on top of the male object. A change in the psychodynamic constellation, when feminine tendencies broke through, was accompanied by a change of the hand-posture.

It was interesting that, in a similar case of a strong, masculine-personality pattern of a woman who was left-handed, the masculine tendency was expressed by holding the left hand on top of the right one. The postural configuration changed after she had turned to an acceptance of the feminine role, when she cupped the left hand with the right one.

It is not my purpose here to enlarge on the psychological problem of handedness. I wish to emphasize, however, that preferred laterality is manifested not only by the hands but by all other organs or functions in which two sides can be involved. Thus, we also find preferred laterality in eyedness, in monocular sighting, in leggedness, and in walking.

The side of cerebral dominance is not inherent nor inherited but is determined by the functional interaction, which means the individual and his environment. The ultimate side of development is a decision which rests with the postnatal functional experience of the individual. This is in complete correspondence with the experiments of Robert Yerkes on chimpanzees. Considering his observations all together and applying them to the human being, we are aware that, in the use of postures of the right and left side of the body, intrinsic and extrinsic factors are involved of which the individual is very often not aware. This is certainly true for the patterned behavior during analysis.

The infant has no laterality preference. He is originally ambilateral. Preferred laterality is a complicated developmental psychologic trait which begins to appear at about nine months. Abram Blau pointed out, from the analytic viewpoint, that the infantile neurosis which leads to negativistic sinistrality appears before the age of 2, and is caused by an unsatisfactory relationship between the child and the mother. He claims that, from the dynamic developmental viewpoint, the sinistral child often develops a rigid, self-willed, obsessive, compulsive, and anal personality structure. In grown-up individuals, sinistrality sometimes may be only a relic of neurosis in the patient.

Many relics of early childhood are transparent in the regressive postural behavior during analysis. They may even indicate the degree of regression. The postural regression may sometimes simulate the very early infantile period, in which the head and neck reflex was still present, *e.g.*, when the arm and leg are both in marked extension and the opposite arm and leg in partial flexion. The tonic neck reflex pattern is a fundamental feature in the mechanics and development of human behavior. Gesell points out that it should be recognized that the tonic-neck reflex behavior pattern is not a stereotyped reflex, but that it is a growing pattern which undergoes progressive changes with the maturity and the economy of the organism. From the standpoint of embryology, all new behavior events have their developmental rules in the old. The tonic neck reflex is, in a broad genetic sense, a movement pattern within which a wide area of postural and high hand coordination are achieved and permanentized. This behavior pattern very often can be recognized in some of the postural behaviors of the adult in the analytic situation. Of course, we meet there only traces of such patterns, but it seems as if the regressive behavior during the analytic situation leads to a regressive postural pattern which has its roots in these early stages of human life, where these patterns became fixed through continual repetition. The mechanism of this repetitivness may be as follows:

All repetitive behavior seems to have its origin in bodily rhythm—apart from the fact that the central nervous system is in a constant state of repetitive neuronal activity. I venture to say that that rhythm can be traced back, as a perceived rhythm, to the earliest rhythm recognized through its sensation, *i.e.*, the rhythm of the heart. The perception of rhythmic breathing movements follows later. The rhythm of the heart represents the most consistent repetitive feeling continually perceived during the whole of life. It might be that repetitive compulsion may stem from these sources. It is well known that any change of the heart rhythm is accompanied by apprehension or anxiety, as if the most secure pillars of the ego were threatened. The child tries to provide its security from the beginning with repetitive acts necessary as a safety device against a loss. As soon as the limbs become integrated into the body-image and become personalized, they are used in their movements toward and away from the body, as a kind of continual check-up on whether the object whose loss is feared is still there. Loss on this early level is always fear of loss of love. The child tries to reassure himself by repetitive movements which can have a compulsive, repetitive character. It selects certain parts of the body for a partial identification with love objects like mother. These parts of the body answer through repetition the fearful question, "Do you love me?" In this early age, the ego is too weak to have sufficient security. It is continually on a try-out through repetitive movements. Later, compulsions are based on such preformed bodily patterns.

The movements and the change of the postural behavior seem to aim at the relief of tension. If the tension is fully and adequately discharged, no repetition should follow, but, if the psychologic stimulus is left undischarged, the postural constellation will be repeated. Therefore, the repetition follows as long as this substitutional discharge is incomplete or inadequate.

From methodologically studied postural behavior patterns, one is impressed time and again with the rigidity of the postural configurations, which apparently represent a defense mechanism, expressed in a habit formation.

This habit formation is, then, the sum total and the combination of involuntary, situationally conditioned positions of the limbs, the head, and the trunk in their relation to each other. This configuration is unconsciously and psychodynamically motivated. The basic posture will always be resumed—disregarding organic or external factors—unless the psychologic situation changes. If this basic posture or postures in a given situation are once firmly established, they will not change, even after very long interruptions of analysis. At least, it was so observed in 30 cases with intervals of from 38 to 59 days, after which the same posture was resumed as had been taken in the last session before the interruption of analysis.

Summary

Every individual has a basic posture. Every posture is meaningful and consists of a combination of different well-determined postures of single parts. All postures of the different parts of the body are attuned to each other, and the change of one partial posture leads to a rearrangement of the total configuration. The movement from one posture to another and the following position of the different limbs to each other is preconditioned by the representation of these parts in the unconscious. The configurations of the posture are bound to be characteristic for the person in question and depend on the firmness of the synthesization of the postural pattern within the personality. Drives and defenses once entrenched firmly in the basic posture lead to a postural defense line, which can be pierced only to a certain degree.

The repetition compulsion is an essential factor in the establishment of the basic posture. During the free associative procedure, unconsciously determined involuntary movements break through. This breakthrough depends on the degree of regression at a certain moment and on the kind of symbolization of the part of the body in question, or on the sort of inhibitory and defensive factors involved.

The posture changes whenever a shift in the psychodynamic situation takes place for which another postural expression is needed. The postural behavior substitutes and may precede the verbalization of the unconscious material. Emerging postures seem to be the product of a long developmental process and have their roots in the earliest childhood. Some betray their early origin by their similarity with infantile postures.

The change of postures seem to aim at the relief of tension. If the psychologic stimulus is insufficiently discharged, the postural configuration will continue or will be repeated. Observations of hand and finger postures show how minutely the postural pattern is laid down. The tenacity of basic postures is clearly proven by their repetition even after a long interruption of analysis.

DIVISION OF MYCOLOGY

PLANT-TISSSUE CULTURE TECHNIQUE FROM THE MYCOLOGICAL VIEWPOINT[*]

By MICHAEL LEVINE[†]

The study of the growth and development *in vitro* of tissues of the higher plants is of recent origin. Though conceived by Haberlandt in 1902, it did not attain a measure of success until the second decade of the present century. Excised roots of the pea and corn plants were first grown in a solution of mineral salts, to which were added organic chemicals and extracts of beef or yeast. The growth of these organs was limited however, and it was not until an attempt was made to subculture these growths that the prolongation of life of these cultures was attained. These cultures were not of a progressive type; they gradually dwindled in size and then failed.

The study of plant tissues or organs *in vitro* remained inactive until after 1930, when this activity became centered around the work of Gautheret and Nobécourt in France and White in America. Gautheret shifted the interest from the growth of organs to the meristematic tissue, the cambium of various trees like the willow, the elm, and others. He succeeded in growing pieces of excised cambium tissue under sterile conditions for considerable lengths of time. Nobécourt succeeded in growing meristematic tissue of various roots and began early to study the needs of the tissues that would insure their growth. White's study led to the first prolonged cultivation of excised fibrous roots. He compounded a synthetic nutrient solution which is widely used and which forms the basis for plant-tissue culture studies. In 1939, he claimed the prolonged growth and development of cultures of undifferentiated tissue.

The relatively short period, 1934 to 1939, witnessed rapid progress in the study of growth, *in vitro*, of plant tissues. Suitable nutrient solutions were improved, the physical conditions of the cultures were investigated, and the technique in the preparations of the primary cultures became more stabilized.

From 1939 to the present, the study of plant tissues took on a new phase. In America, pathological tissues, such as those derived from the callus of the tobacco hybrid (*Nicotiana glauca* x *N. Langsdorffii*) and the tumors of the crown-gall disease of the sunflower, were studied principally. The calluses of the tobacco hybrid are spontaneous globular growths with leafy elements which appear on the root, stem, and pedicels of the maturing plant. Contaminants are present, but they are limited to the surface of this overgrowth. The crown-gall tissue, studied mainly by White, Braun, Riker, and DeRopp, represents a sporadic secondary tumor on the sunflower which is free of the tumor-inciting bacteria.

The crown-gall disease is induced by piercing a chosen part of a plant with a needle that has been immersed in a subculture of *Phytomonas tumefaciens*. Tumorous growths appear in the region of inoculation. The bacteria, though not seen in microscopic sections of the tumor, grow rapidly on the agar medium when fragments are appropriately seeded. The bacteria-free crown-gall tissue has been used extensively for physiological studies by Riker and his students and DeRopp.

In this laboratory, the so-called "non-pathological," frequently referred to as the "healthy," tissue and the pathological type have been the subjects of our endeavor. These tissues were cultivated *in vitro* so that their growth was attained to the exclusion of bacteria and other mycotic organisms. Segments of stems and of fleshy roots, together with small plugs of tissue (2 mm x 3 mm) from the meristem of roots of a long list of plants, were seeded on nutrient agar. The technique employed has yielded tissue cultures of the carrot, tobacco, sunflower, Jerusalem artichoke, parsnip, salsify, and kohlrabi. Many plants yielded poorly to *in vitro* growth, principally because their contaminants thrived on the medium better than the host tissues.

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[†] Laboratory Division, Montefiore Hospital, New York, New York These studies were made possible by grants-in-aid from the American Cancer Society, Inc , through the Committee on Growth of the National Research Council

The techniques employed in the preparation and study of plant-tissue cultures, as elaborated by White, require extensive facilities, such as air-conditioned chambers for storage of cultures, medium room, transfer room, and laboratory for preparation of nutrients. On the other hand, the European methods are simple and inexpensive. The open laboratory, with sterile instruments and paper, a gas flame, and a beaker of alcohol constitute the chief accessories to the successful induction and transfer of tissue growths.

Subcultures are made in a small glass wall-chamber, the size of which permits the free use of both forearms and room for as many as six to eight 125 ml. flasks at one time. Sterilization of instruments is accomplished by water kept at boiling point by an immersion heater. The hands are repeatedly washed and rinsed in alcohol. Erlenmeyer flasks, with medium and tissue cultures, are kept on racks in the laboratory. These are covered with sheets of cellophane so that the shelves are completely enclosed.

The organs of the higher plants have been considered "healthy." Since 1936, the notion prevails that these plants, though healthy in appearance, not only have ever-present surface contaminants, but also harbor many types of microorganisms in their internal tissues. The most recent study (Tervet and Hollis) shows that plugs of tissue of the common underground stems and roots like the potato, carrot, turnip, *etc.* yield bacteria when placed in sterile water. Precautions have been taken to sterilize the surface of the organs. Clostridial forms and Gram-negative rods are common. In our studies, entire sets of experiments were frequently discarded owing to contaminants residing in the primary inocula. These bacteria seem to grow better, even on White's medium, which elicits slow growth-reactions from the seeded tissue. Frequent transfer, with the hope of eliminating the invaders, rarely proves effective. The use of such material invariably results in loss of time and medium.

The carrot taproot and other structures used to initiate and develop tissue cultures were carefully selected. Taproots were bought in the open market and planted in beds in the green house. When leaves had developed, they were removed, washed in running water over night, scrubbed, and surface sterilized by immersion in chlorox or rose-x in proportions of 1:4 for one hour. They were then washed in running water again, rinsed abundantly in distilled water, and finally rinsed in alcohol. The root was brought to the transfer chamber and broken into two, and, with the aid of sterile cork-borers or trocar, appropriate lengths of tissue of a given diameter were removed from the generative zone and placed between sterile sheets of paper in the chamber. The tissue was then cut into desired lengths. As many as six inocula were then placed on the surface of 25 ml. of a sterile medium in chemically- and heat-sterilized 125 ml. flasks.

Stems of tobacco (*N. tabacum*, *N. affinis*, *N. Langsdorffii*) and the hybrid tobacco, sunflower, and Jerusalem artichoke stem, were all grown in the green house. After their removal from the plant, they were treated as just outlined. To insure removal of surface contaminant, the outer epidermis was removed under the protection of the transfer chamber. The stems were then cut into segments several mm. thick and seeded. Many of these segments yielded bacterial contaminants.

Crown gall induced by *P. tumefaciens* on the stems of sunflower, *Bryophyllum*, and kohlrabi and roots of salsify, parsnip, and carrot yielded no actively growing tissue cultures after treatment with the antibacterial products such as penicillin, streptomycin, aureomycin, and tyrothrycin. Surface sterilization of sunflower and *Bryophyllum* crown galls by sodium hypochlorite has yielded slow-growing tissues free of the tumor-inciting bacteria.

The cell of healthy plugs of root and segments of stems begin to proliferate. They overgrow the surface of the inoculum and spread to the medium. This tissue is removed from the original inoculum after several months and transferred to fresh medium, where growth proceeds until sufficient tissue is formed to make a number of subcultures.

These new cultures represent fundamental tissue growths. It is this type of growth that has been studied in this laboratory. In the process of dividing, transferring, and final storage of this tissue, contaminations by molds, yeast, and bacteria occur. The loss in number is small, but the time lost may be exceedingly great.

Cultures of tissue derived from the carrot taproot and grown *in vitro* have been intensively studied by Gautheret and Nobécourt for more than a decade. They pointed out independently the need this tissue has for the plant growth substance, indoleacetic acid (heteroauxin). This substance, added to the mineral salts, sucrose thiamine, and oligodynamic elements, enables the carrot tissue to grow.

In 1946, fragments of carrot tissues were started on a mineral-salts-and-sucrose substrate, and the tissue masses derived were primarily used to study the effect of the chemical carcinogens. Three such chemicals were used: 1,2,5.6-dibenzanthracene; 3,4-benzypyrene; and 20-methylcholanthrene. The new tissue growths were so divided as to form five cultures: one was grown on a standard medium; another incorporated in the same medium, indoleacetic acid at a concentration of (5×10^{-6}) 0.5 mg. per cent; and the other three each contained, besides the standard medium, a definite concentration of one of the carcinogens.

Tissues grown for several transplant generations on heteroauxin and then transferred to standard nutrient or medium, in which dibenzanthracene had been incorporated, began to show evidence of shoot formation with roots. This occurred eight months after the primary cultures were started. Within a period of a year, the cultures produced complete carrot plantlets. Besides showing morphological and physiological differentiation, these tissue masses, following growth on heteroauxin, presented characteristic changes common to the crown-fall disease. Similar responses were elicited by the chemical carcinogens. Growth of the tissue appeared to be stimulated.

Cytological studies of the plantlets revealed tissue structures not unlike those induced in plants by the crown-gall organism. It is believed that the response is not one of crown gall but that of a protective mechanism exhibited by the plant regardless of the source of injury, whether a parasite or a chemical agent. Segments of the tobacco *N. Langsdorffii* and *N. affinis* produced tissue masses which yielded complete plantlets.

In subsequent studies, which are to be reported elsewhere, fibrous roots derived from pedigreed cultures and whole segments of carrot roots have been observed to form tissue masses from which completely differentiated carrot plantlets were formed.

Summary

(1) Meristematic or generative tissue of higher plants may be grown on pure synthetic medium to form tissue cultures with unlimited capacity to proliferate.

(2) Tissues of healthy higher plants show bacteria gain access to the tissue most likely through small wounds or lenticels.

(3) Careful selections of organs, surface sterility, removal of surface epidermis, and use of the inner tissue reduce contamination most frequently caused by bacteria.

(4) Division and transfer of the tissue culture expose it to contamination by molds, of which *Penicillium sp.* is most common.

(5) Tissue cultures of the carrot taproot and segments of tobacco stems (*N. Langsdorffii* and *N. affinis*) yielded completely differentiated plantlets.

(6) Cytological studies of the tissue masses and some plantlets of the carrot

examined consist of tissues not unlike crown gall after growth on the chemical carcinogens.

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SECTION OF ANTHROPOLOGY

LIFE IN THE EARLY ISLAMIC CITY OF NISHAPUR, PERSIA*

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The acquisition of objects of value by digging for them on ancient sites has a long but not particularly honorable past. Looting and learning have often gone hand in hand, with sometimes the one and sometimes the other taking the lead. Eventually, archaeological digging was done with the express intention of corroborating history as it was known from literary sources, such as the Odyssey and the Bible, and much work was done to that end at Troy and in Assyria, Egypt, and Palestine. Nowadays, some of our knowledge of early man in the remote past is being gained almost entirely from archaeological excavations. We must remember, however, that the archaeologist is always in danger of basing too much on too little. Even museum curators have been naïve enough to believe that digging surely will give them definite answers to embarrassing questions of date and provenience. It seems that archaeology, from being an acquisitive child (and, like all children, none too careful of how and where it gets things), has come to be considered an oracle.

To have some understanding of the past without losing oneself in it is a reasonable and profitable intellectual pursuit, though we know that each age can only see the past in its own peculiar and limited way. It is well, however, to look at that past as honestly and completely as possible and to avoid Oliver Goldsmith's indictment, "But of all the learned, those who pretend to investigate remote antiquity have least to plead in their own defence when they carry this passion to a faulty excess. They are generally found to supply by conjecture the want of record, and then by perseverance are wrought up into a confidence of the truth of opinions which even to themselves at first appeared founded only in imagination."

The part that archaeological excavation can play in this examination of the past is distinctly limited, and, if any clear understanding is wanted, it is necessary to supplement that work by information from other sources and by other means. Literature, the visual arts of representation, ethnography, and anthropology all can be of assistance to this end, but each of these will prove incomplete and unsatisfying if pursued solely along its own lines.

In literature, some things are never written; in painting, some things are never depicted; and Time sees to it that, no matter how carefully an ancient site is excavated, only a certain amount of the original material remains. The result is that, willy nilly, false emphasis is placed on that which is preserved. The objects found may be and often are in puzzling disorder. The living people in the locality may and frequently do reflect much of those who lived before them, but it is often exceedingly difficult to judge just how the old and new habits and customs are intertwined.

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The purpose of this paper is to describe some aspects of the early Islamic city of Nishapur and those who dwelt within it. To this end, I have combined information gathered from sources ancient and modern, from the literature of the past, from observation of present-day life, and from the excavations made by the Metropolitan Museum.

The reports written of Nishapur in early Islamic times contain much contradictory material. This need hardly be a matter of surprise, for there is no reason to think that those who visited foreign parts and strange places in ancient times were any more exact in their reports or less affected by their emotions and personal peculiarities than they are today. It was just as possible then as it is now to damn a city or a whole country because of an unfortunate encounter with a tough and unsympathetic official. The following quotation from the poet Muradi is, I am sure, an example of this: "Be careful not to go to Nishapur if you are not closely protected by the sultan, for in this city neither merit nor high birth is a safeguard, and the respects due to a man are not recognized." The early literary records run the whole gamut from praise to blame. Anwari, a famous poet of the late twelfth century said: "How excellent is the city of Nishapur which is upon the face of the earth: if there be a Paradise it is here, and if it be not this then it does not exist." Another man, most extravagant in his praise of Nishapur, gives himself away when he says, "and there I found the most beautiful creation of Allah—in the form of a Turkish slave." At the other extreme we have a learned man who, alas, as some learned men do, got a little sour: "For six years," said he, "science and culture have kept me imprisoned in the dust heap of Nishapur." As this man goes on to say "I have so excelled in all the accomplishments that anyone has mentioned, that in all the world I have no second," we can consider his powers of judgement distinctly warped. Some writers show a better sense of proportion. Of Rayy, once a great Persian city near Teheran and on the main road to Nishapur, Ibn Haukal wrote in the tenth century, "except for Baghdad it is indeed the finest city of the whole East though Nishapur in Khurasan is more spacious."

At present, Nishapur is a small town about ninety miles southwest of Mashhad on the main road that goes from Teheran to Afghanistan. This road is one of the great highways of Asia and has been called the "Khurasan road" and the "great silk road." Khurasan is now the northwest province of Iran, but in earlier Muslim times the name, which means the land of the Rising Sun, was applied to a much vaster area, which included both Bokhara and Merv. The name was sometimes used to designate the land right up to the Oxus river. Nishapur was, on occasion, the capital city of this vast area, though other cities, such as those I have mentioned, were effective and usually successful rivals.

Nowadays, Nishapur is mostly a by-stop that caters to the pilgrim tide that flows to the shrine of the Imam Riza, the eighth imam, at Mashhad. The Imam Riza, much revered in Persia, died in the beginning of the ninth century, supposedly poisoned by the Caliph Mamūn, son of Haroun er Rashid. Both the Imam Riza and Haroun er Rashid are buried in Mashhad, the Imam Riza in a silver shrine, which is almost worshiped, while that of Haroun er Rashid is neglected, if not actually desecrated. Each, however, has become more or less immortal, the one through the religious faith of Shi'ite Muslims and the other through the fabulous but highly informative fables known to us all as "The Thousand and One Nights."

Nishapur existed as a city before either of these men lived. Its very name betrays its Sasanian origin, for Shapur was the name of several Sasanian kings. We are not now concerned with the Sasanian city, however, and no traces of it were found in the excavation conducted by the Metropolitan Museum. There is every reason to believe that the Sasanian city continued to be used for at least a whole century after its capture by the Arabs. Then a new city was developed in a suburb, usually referred to as Shadyiakh, some miles away. Exactly this sort of thing happened at Shiraz, where the museum also worked, except that there it was the Sasanian fortress and adajacent town that were excavated. Cities in Persia were often rebuilt several miles away from their original sites, and suburbs can be considered to mean villages within thirty miles in the same plain. In Nishapur, no buildings found by the museum could be definitely dated before the beginning of the ninth century, though some may have been erected in the late eighth century. Much was probably built by ʻAbdallah ibn Ṭahir and by ʻAmr ibn Lais who wished, according to report, to conquer a place where the grass was rhubarb, the earth was covered with vegetation, and the stones were turquoises. These qualifications may seem utterly fantastic to anyone unacquainted with Nishapur; yet Nishapur is still famous for its rhubarb and the syrup which is made from it. The plain is still exceedingly fertile and the turquoises found in the mines a few miles away are still the finest obtainable.

The city that flourished in the early days of Islam now lies beneath a vast area of land, which is cultivated even though it is thick with fragments of ancient brick and pottery. The flatness of the plain is broken here and there by the ruins of walls and by hillocks formed of the old buildings—the sun-dried bricks have again become the earth from which they were made. Dominating the site is a great mound, known as Teppeh Alp Arslan, named after the famous Saljuk Turkish conqueror of the eleventh century.

This huge mound was erected in the ninth century and formed the platform on which the citadel was built. It is not a mound composed of the accumulated detritus of many ages. This practice of making raised earthen platforms was not confined to Nishapur; the citadels at Bokhara and Samarkand are raised above the plain in the same fashion. The top of the mound was covered with small dwelling places, following the pattern set by the Sasanian fortresses, such as that as Kasr-i-Abu Nasr at Shiraz. The fortress was used until the thirteenth century, when, after the Mongols sacked the town, it appears to have been abondoned. The citadel and the town were enclosed by walls made of trodden, puddled earth. At the citadel, there is some evidence of its having been reinforced with stones.

The walls were pierced by two and four gates respectively, that on the East being called ominously the War Gate. Wars were frequent, and the city changed hands repeatedly between the ninth and the thirteenth centuries. Several of these affairs were exceedingly bloody and we read of slaughter even in the great mosque itself, with the floor covered with bodies and invisible from the blood in which they lay. Persians, Arabs, Turks, and Mongols all used the sword ruthlessly in this city. The chief blame seems to rest on the Mongols, who built separate pyramids of the heads of men, women, and children. In any case, the resurrections of the city were extraordinary despite wars, massacres, and terrible earthquakes. In 1267, Pur-i-Baha describes broken minarets, libraries upside down and colleges all forsaken, the Friday mosque in ruins and the pulpits shattered. In 1270, he speaks of old Nishapur being young again, like a dotard regaining his boyhood. This reference alone shows that even before the earthquake of 1280 the city had recovered to no small extent from the Mongol invasion.

There undoubtedly were caravanserais near the gates in ancient times. Long strings of camels came there, rested, and went. Places for the same use still exist, for the traffic of motor trucks has by no means entirely displaced the heavy and powerful camels of the region. The old khans, too, that opened off the narrow streets of the bazaar, were probably not much different from those of today. The ancient bazaar was built in the form of a cross and, as is usual in the Near East, was divided into sections, according to the nature of the goods sold and made there. Nishapur was especially famous for its silk and cotton weaving. There were, however, many other things locally made and, in addition, many imported goods from China in the east and from Iraq, if not from even further to the west.

Near the encircling walls and by some of the largest buildings were great depressions in the ground from which clay had been taken for making bricks. We found such depressions in our excavations. In early Islamic times, most bricks were sun-dried and the buildings, when complete, were usually covered with a coating of mud and chopped straw, which in some instances was covered with a thin coat of white plaster. Generally, the city must have been of a dun color, with some of the larger and more important buildings glistening white. It is probable that, on certain occasions, some buildings were just whitewashed before the arrival of important persons, as is often the case in the country in the present century.

Kiln-fired bricks were also used, the kilns being near the walls or even outside them. The fire burned the clay a bright yellow, as is the case at the present time. There were exceptions to this, however, and in the Saljuk period, *i.e.*, the eleventh and twelfth centuries, elaborately carved bricks were fired to a dull red color. These special bricks were of huge size and were carved with inscriptions and ornament. They were used as decorative bands on the surfaces of some of the mosques and other such buildings. Our excavations revealed a detail that had not previously been known. The background of such cut and carved bricks was colored bright blue or a vivid red from a pigment containing cinnabar. It is probable that the decorated surface was painted white. Also introduced in Saljuk times was another form of brick and tile decoration in which the brick, instead of being made of a natural clay, was made of a composed body and was covered with a bright blue alkaline glaze. There were two intermediate techniques—though they were all contemporary. In one, the tops of the letters were glazed blue and the background and other decoration were painted red; in the other, scraps of flat blue glaze were fitted into holes cut in the bricks. This introduction of the glazed brick and tile was a resurrection of an art the Persians had learned from the Babylonians and which they practiced during the Achaemenian period. This was an art of the so-called "barbarians" which was killed by the Greeks. Evidently the Greeks could kill arts as well as sow them.

There were many mosques in the city. One of the most important was built by Amr ibn al Laith toward the end of the ninth century. We have a fairly full description ot it, but it cannot be verified, as the remains have yet to be discovered. It appears to have had three arcades around a great court and eleven gates flanked by marble columns. The chances are fairly good that the piers of the arcade were like those of the mosque of Ibn Tulun in Cairo, which was related in style to the famous mosque at Samaria in Iraq, a ninth-century building. I think this is likely, for some piers of the same type, and certainly of the ninth century, were uncovered in our excavations. Some of the mosques were of considerable size. We read, for example, that the mosque of Mutarriz, which was sacked by the Ghuzz Turks, held 2000 persons. On the other hand, other places of worship were exceedingly small. A feature of Nishapur was the introduction of shallow plaster prayer niches into the rooms of small dwelling places.

Some of the most important buildings in Nishapur were the religious colleges, known as *madrasehs*. The word unfortunately has become associated with a particular type of building in which four vaulted recesses form the sides of a rectangular open court. There is no evidence that this kind of *madraseh* was ever built in Nishapur. There were many *madrasehs* in the city and one of the first great Saljuk *madrasehs* was built there. At this time, the Sunni form of Islam was the official one, and the special form taught in the *madrasehs* of Nishapur was the Shafí doctrine. Religion and politics as well as religion and law were most closely allied. The Nizam el Mulk was the great statesman or wazir of the eleventh century. He endeavored, by means of the *madrasehs*, or colleges, to achieve some cohesion in the vast state under Saljuk rule and to combat the propaganda of the Shiá sect and other dissidents who were active in the *khanegahs*, or cloisters. We have records of quite a number of the doctors and jurisprudents of Nishapur who taught in these colleges, of which, according to Nasr-i-Khusrau, there were no fewer than seventeen in Nishapur alone. The most famous of these teachers, and the one whose influence has probably lasted longest, is Ghazzali, whose writings are supposed to have influenced Thomas Aquinas. His works are still read and his ideas are still actively discussed both in Persia and elsewhere.

There are many literary references to scholarship in Nishapur. One of the earliest concerns 'Abdallah ibn Tahir, the great governor of the early ninth century, who, though he ordered the destruction of an illuminated Magian book, is quoted as saying, "Knowledge must be accessible to the worthy and the unworthy: knowledge will look after itself and not remain with the unworthy."

About the end of the tenth century a book was written by a Nishapur scholar named Jauhari entitled *Tarikh-i-Ulema-Nishabour*—which was a history of the doctors of Nishapur, but the work unfortunately has disappeared, though it was used by later writers, more particularly by Ibn Khallikan in his biographical dictionary written about 1250 A.D. From this latter book and from the *Tarikh-i-alfī*, the Millennial History, written in 1588, we get some sidelights on the learned men of Nishapur and amongst them an incident in which Omar Khayyam, the great mathematician, and poet, plays the leading role. Omar Khayyam was supposed to believe in the doctrine of metempsychosis. One day when an old college was being repaired, he was walking by with a group of students and saw that one of the donkeys bringing bricks would on no account enter the *madraseh*. Omar Khayyam smiled, went up to the donkey and recited extemporaneously the following quatrain:

> "Oh lost and now returned yet more astray,
> Thy name from men's remembrance passed away,
> Thy nails have now combined to form thy hoofs,
> Thy tail's a beard turned round the other way."

The donkey then entered and the students asked why his verse had such a magical effect. Omar Khayyam smiled and said, "The spirit which has now attached itself to the body of this ass formerly inhabited the body of a lecturer in this college, therefore it would not come in until now when, perceiving that its colleagues recognized it, it was obliged to step inside."

From the ninth to the twelfth centuries there was much disputation on religious matters, but the strife, though sometimes vehement and deadly, was not more bloody than in Christendom, perhaps even less so. Nishapur was no exception and was a hotbed for various sects. There were several tendencies at work. The Muslims who believed the Koran was created found it not incompatible with Greek philosophy. They were gradually taking second place to those Muslims who took the Koran literally and who, considering it divine, were logically driven to maintain it existed before it was written. There were many 'Alids and they formed the Shiá or faction, that division of Islam which believes that the Caliphate should have remained in the family of Muhammed. It is to this faction that the great majority of Persians now belong, but during almost the whole of the early period of Nishapur's history it was to the orthodox or Sunni sect that its rulers belonged.

Another religious force strong in Persia and Nishapur was Sufism, which gave a mystical turn to Islam. The Sufis believed not only that there is no God but God, but that there is nothing but God. Al Hallaj, who was born in Khurasan, speaking of God said, "Betwixt me and Thee there lingers an 'It is I' that torments me. Ah, of Thy Grace, take away this I from between us." It was indeed taken away from him, for, by maintaining that he was the Truth (which was considered tantamount to saying that he was God), his fate was sealed and he was beheaded. Some of the greatest minds were influenced by the Sufis, one of the most important of those who taught at Nishapur being Ghazzali. He, however, did not go to such lengths as Al Hallaj. One reference to a book known as the *Mishkat al Anwar*—the niche for lights—written by Al Ghazzali will give an indication of the hair splitting and logic chopping that went on in Nishapur and other centers of learning. He quotes a couplet from Al Hallaj which runs:

"I am He whom I love and He whom I love is I.
We are two spirits immanent in one body."

He treats this critically and then gives another verse:

"The glass is thin, the wine is clear,
The twain are alike. The matter is perplexed;
For 'tis as though there were wine and no wine glass there,
Or as though there were wine glass and nought of wine."

He then points out that there is a difference between saying, "The wine is the wine glass" and "it is as though it were the wine glass," and he indicates that a man unconscious of his own consciousness is in a state called in the language of metaphor, identity, and in the language of reality, unification.

In many of these religious matters, however, political interests were often involved, and it is not easy to assess how much the theological strife of the day affected the learned circles of the city. For the general population, it probably made little difference. The one thing that seems certain is that most of the time there was room for a considerable difference of opinion between the various sects of Islam. Some of their various adherents could, when things got too hot, avail themselves of the art of dissembling their true beliefs without falling from grace, while outwardly conforming with that which was publicly professed. Even between the adherents of different religions enmity was not too great, for Abu Muslim, the great and unscrupulous champion of the Abbasids, nevertheless was a friend of Sinbad of Nishapur, a Zoroastrian, who, after Abu Muslim's death, attempted a revolution with fatal results, his death being due to his political activities.

Although there were so many mosques, *madrasehs*, and convents in Nishapur, we were reminded by several finds in the excavations that there were other religious faiths in the early city. Several objects were found which were decorated with crosses, as for example, plaques and glazed pottery. It was quite obvious that the crosses on these things were used as a Christian symbol and not as a haphazard decorative device. Records have come down to tell us that there were enough Christians of the Nestorian faith in Nishapur to have their own bishop.

During the ninth and tenth centuries there were still many Zoroastrians, for not until the eighth century was there any major adoption of Islam.

Mani lived in the third century after Christ and founded a dualistic religion. His followers, the Manichaeans, spread his faith in Europe and Asia and were persecuted in Christendom and Islam. Some of them, too, were probably in Nishapur, with their numbers being temporarily increased during the tenth century, when they were driven out of Mesopotamia and obtained temporary refuge in Khurasan before proceeding further East to Turkistan, where remains of their paintings have been preserved. They were famous for their proficiency in the art of painting and it is most probable that their influence was felt in Nishapur. Be that as it may, we have material evidence that wall painting was practiced in the beginning of the ninth century. Unfortunately, there were but few remains showing human figures. These were discovered all broken up at the bottom of a pit near a house The large figures had haloes and there were demons included in the scene, but not enough was found to give the key to the subject.

In one figure, the head of a woman, two peculiarities may be noted: (1) the very small size of the mouth, which fulfills the mark of beauty as favored in Persian poetry—"a mouth smaller than an eye" (the English equivalent is a "rosebud" mouth); and (2) the two black spots beneath the eyes. These last are not tears, but are either tattoo marks or, more probably artificially induced scars. The practice in Iran seems to have gone back to Parthian times, and perhaps to long before that. As Dr. Schuster points out, however, the custom has persisted among the Mongols. To my mind, this curious custom is perhaps related to the prevalence of the *salaq*, the circular scar left by a boil that often breaks out on the cheek, to which the inhabitants of the Near East and Turkistan are very susceptible. It is not inconceivable that a blemish was changed from a stigma to an artificial beauty spot by reproducing it on the other cheek in much the same way as has been done in the west with moles and patches. In any case, this strange fashion is in no way peculiar to Nishapur; it was and is widespread.

Other paintings found in their original positions on the wall contain no human figures but, in addition to elements that are purely decorative, there are others which obviously once conveyed some special meaning. On one panel, for instance, can be seen pairs of groping hands, each with a circular device on the palm. In another, the hands stretch out to small pyramids of fruit or to a black disc. These features are not to be found in any of the ninth-century paintings that have been found so far, in Iraq or elsewhere. In small elements from the painted plaster filling of a squinch of the ninth century, two prominent eyes appear. The use of this motif is very widespread both in time and place. It is still one of the peculiarities of small Mediterranean ships. In early Nishapur, it occurs not only in wall paintings but on a number of small metal objects, and we are reminded that the Scythians and other nomad tribes near this area used this symbol frequently. Their use can be associated with no one religion but would seem rather to be evidence of some superstition common to large groups of men. In Nishapur, not only was the eye itself depicted in this particular way, but charms were worn to defend the wearer against the evil eye. In early Saljuk times, it was possible to make very cheaply bright blue glazed pottery amulets which resembled turquoise, the efficacy of which against the evil eye has been held paramount for centuries. As defense against the same threat, charms were also written from the Koran and, in the case of the well-to-do, placed in cylindrical containers such as one of silver gilt, with an inscription in niello work, which has been preserved from the early ninth century. It was made to be suspended from a cord encircling the neck. In this instance, the inscription is a particularly potent one, for it is the chapter of unity and its recitation was equal in merit to the reading of a third of the whole Koran.

Another indication of unorthodox beliefs or myths was found in the form of some broken molds made of well-levigated clay. The subject of a bird (here perhaps human-headed) flying upwards, with a human figure against its body, is, of course well known, but it goes back to long before Greek times, as far back indeed as about 3000 B. C. in ancient Sumaria. The subject crops up again and again. It figures in the Shahnameh in the story of the albino hero named Zal.

Concerning more material things, a

matter of the utmost importance in the relatively dry region of Khurasan was the procurement of water. Several early writers have remarks to make about the water supply of Nishapur. They note, for example, that it was rather far below the surface of the earth and, according to at least one report, one could descend as many as 100 steps to reach it. The longest flight of stairs to a source of water that was revealed by the excavations was 33 steps and the 100 can probably be taken for an exaggeration. Water channels did pass deep beneath the surface of the city, and one dissatisfied visitor suggested that Nishapur would have been a much nicer place if the water had been on the surface and the inhabitants down below.

The water was brought from the near-by mountains by means of underground aqueducts in the form of small tunnels lined with pottery hoops shaped much like horse collars. These hoops are still being made, in kilns on the outskirts of the modern town. The water eventually came out at surface level south of the city and was used to irrigate the land. This means of conducting water has a long history in Iran and goes back to at least the first century. Its distribution was always a most fruitful source of trouble. Recognizing this, 'Abdallah ibn Tahir, who built part of Nishapur in the early ninth century, had the "Book of Canals" written. It served as a guide for the legal distribution of water for at least two centuries. There are literary references to reservoirs of water at Nishapur, but, apart from a few small pools, their number would seem to have been in no way exceptional. It would seem rather that the main way of getting water was from subterranean chambers, through which the underground watercourse passed. The finest one was a domed octagonal structure built of kiln-fired brick, with seven recesses and entered by a steep flight of stairs. The water emerged from the tunnel and flowed into and from a tank into which women could dip their pitchers and men their goat skins. There were also innumerable wells, often in very close proximity to latrine pits. I believe that the minimum distance considered necessary to insure absence of pollution is at least 200 yards. In Nishapur, the distance was nearer 2 than 200. The wells were often, but not always, in open courts on which rooms of the house opened.

The houses themselves were usually built of sun-dried brick and, apart from the poorest hovels, were covered within by coats of fine white plaster, which were renewed when necessary. The floor also was covered with white plaster and, in order to keep it clean, there was often, in the more luxurious houses, a general raising of the floor except for a rectangular space adjoining the actual doorway. This area was sometimes provided with a small drain hole. Another feature that seems to be peculiar to ancient Nishapur was the intrusion of a sunken pot in the center of the floor. The orifice was usually surrounded by a small rectangular moulding. Charcoal was placed in the vessel and air was introduced at the bottom by means of an inclined pottery pipe. It might be wondered how such a small amount of fuel could possibly heat a whole room, and of course it could do no such thing. This central sunken charcoal fire was used as the Persians in the country still use a charcoal brazier. Over it is placed a wooden framework known as a *kursi*, and over this structure quilts are thrown. People then sit or lie around, with most of themselves beneath the covers. It is a most ingenious, simple, and economical way of keeping warm. But as a Persian once said to me, "It is very pleasant indeed to sit beneath a kursi, but one doesn't get much work done."

In connection with the sunken fireplace, the answer was found to a troublesome question which arose concerning a number of stone animals that had been found in the excavations. These animals had one feature in common—a deep notch in the middle of the back. A brazier of the late thirteenth century, now in the Metropolitan Museum, shows special hooks at the sides for holding a spit when the brazier was used for grilling. A miniature in the Museum of Fine Arts at Boston shows a picnic in the open air, and one can see that little metal spikes of a similar shape had been put in the ground for this same purpose—a development of a simple forked twig. Such spikes were not driven in the plaster floor and the notched stone animals took their place, being placed on either side of the sunken fire to support the spit.

On the plaster floors, rugs were scattered, which, with their brightly colored wools, introduced some color into the white interiors of the houses. Just before the close of our work in Nishapur, in examining a large room of an important building, we discovered that the original plaster floor (for it was composed of many successive layers) had been painted brightly in red and blue, a feature not hitherto encountered. Unfortunately, it was so damaged that it is impossible for me to give any idea of the pattern with which it was adorned. It hardly seems a very practical idea; yet the evidence is incontrovertible.

The most gaily painted places were bath houses, both public and private, as far as we are able to tell. Considerable license has always been practiced in Muhammedan countries in the decoration of these places. They were replastered and repainted over and over again. One that we excavated had lively scenes, though unfortunately it was possible only to guess at most of them. Animals, birds, human figures, and winged angels were painted on the walls. There is no doubt whatever that the miniature paintings that have been preserved from the thirteenth- and fourteenth-century bestiaries, of which pages exist in the Morgan Library and the Metropolitan Museum of Art, followed this earlier tradition of painting in direct line.

The fact that people usually sat on rugs on the floor had its effect on the manner in which rooms were decorated. It was a perfectly natural and sensible thing to have the wall decoration, if there was any at all, in a band about a yard high, forming a dado. These dadoes were sometimes painted and they were composed of carved plaster. The decoration was cut directly in the plaster after it had been applied to the wall. The Persians were, and still are, very skillful at this work and succeeded in making most interesting patterns of light and dark, filling in regular geometrical shapes with varied but closely related ornament. The leafy forms are highly conventional, but they are treated with much ingenuity. Other elements, such as bird's heads, are often introduced, showing that the tradition of the north Persian art of Luristan and of the Scythians was anything but dead. The main borders of these plaster panels were usually painted a golden ochre, and the backgrounds of inscriptions, which were always in Arabic and never in Persian, were painted blue.

By means of plaster, the inhabitants of early Nishapur were able to have small glass windows. The glass has a yellowish tinge and is always undecorated when used for this purpose. The technique of setting glass in plaster in the Near East has survived until the present day, but, in western Europe, weather conditions did not allow it to be used and the practice of setting glass in lead calms took its place. The use of glass was widespread and many fine pieces have survived, although most of them are broken. Engraved plates and cut-glass bottles show great technical skill. The bottles and decanters were used for wine, which was made and drunk in Nishapur.

Although some glassware was probably imported, glass was made in Nishapur itself. Some of the glass of the ninth and tenth centuries was of very fine quality. Here is a verse of the tenth-century poet, Daqiqi, which illustrates the point:

Water and ice, behold within a glass
How candle-bright all three do shine, but see,
The twain will fuse, yet one a frozen mass
Remains. One hue, one luster have these three.

Some of the glass found in Nishapur really was as colorless as ice. Glass was used for medical purposes, and we found traces of this at Nishapur in the form of several cupping glasses. They were operated by sucking the stalk-like tube that is annealed to the cup itself. Medical knowledge in the Muhammedan world at this time was far more advanced than it was in Europe, and Persia was well in the vanguard.

For domestic use, glazed pottery was far more common than glass, and most of it was made locally in domed kilns. Vessels of various shapes were made of it: cups, dishes, platters, bowls, and pitchers. Relatively few were plates, due to the fact that food was eaten with the right hand and knives and forks were not used. The hands, I should like to point out, were washed before and after eating in all decent circles, with rose water in the really elegant ones. Pottery was imported from both China and Iraq during the ninth and tenth centuries. In the tenth century, there seem to have been importations from other cities in Khurasan. The pottery from China must always have been rare and costly. From Iraq came the highly prized lusterware which the potters of Nishapur were never able to make, at least until the thirteenth century. That it was well thought of is proved, not only by the presence of the imported ware itself, but by the imitations made of it. As the technique for making these copies is entirely different from the original one, it is no wonder that the imitations bear only the most superficial resemblance to the true luster pieces and that the metallic iridescence is entirely lacking.

In underglaze painting, the potters of Nishapur produced some of their most effective achievements with an amazingly economical use of color and decoration. Using merely black and white, with an occasional touch or two of red, the results were, I think very satisfying and conform to our present-day taste.

From the Nishapurians' standpoint, a ware strongly influenced by Chinese glazed earthenware of the T'ang period was equally if not more popular. It has green and yellow splashes and often spots of purplish black on a white ground. More or less obliterated by these colors are scratched designs which are often of considerable delicacy.

Another style confined to Nishapur and to the north of Khurasan by the Caspian sea is remarkable for the extraordinary birds, beasts, and human beings that figure upon it. Apart from one other ware made in later times and the wall paintings, here alone do we have material evidence of the appearance of the people living in the city. From one bowl, we probably have some idea of how women dressed—presuming the figure to be a woman and not a young man. A curious cloak with divided tails seems to be peculiar to Khurasan, but this may be due to lack of information from other regions. On another bowl, leather leggings worn by a man may be taken as a definite part of the costume of a man of the upper classes. Also, he is drinking a glass of wine. Although in these drawings the beakers are usually shown with concave sides, those they actually used were tapering but with straight sides.

Fantastic though some of the bowl decoration may seem to be, it should not be cast out too quickly. Men did, for example, wear flowers in their hair, if accounts of certain convivial gatherings are correct. An animal sitting behind a horseman, strange and winged though he be, reminds us that hunting with cheetahs was practiced in Khurasan until the seventeenth century, the animal being trained to catch the prey and to sit behind his master's back on the horse's rump. A wall painting shows another method of hunting also still practiced in the near East and in Turkestan—hunting with a falcon. Most of the details of this ninth-century painting must be considered accurate, even the metal tags on the dangling ends of the man's leather belt. The woven roundels in a man's dress were worn throughout the Saljuk period in Persia and also in the corresponding period in Egypt. None of the old garments were found in the ruins, save the metal tags just mentioned and ivory and bone buttons. These buttons were often extremely handsome and were cut and inlaid with color. There are no references to them in any writing, however, and no miniature shows how or where they were used. They have been found in hundreds of early Islamic sites from Egypt to east Persia, but only in Nishapur do we have any inkling as to how they were used. It appears that they were used in pairs—one large and one small, being joined by an iron haft. So far as I know, it is not a feature of any near Eastern dress, Arab or Persian, at this time. It is possible that the buttons were used in other ways also. Thus, we are almost as far away from the solution of the problems as we were before.

I mentioned previously another type of pottery which was introduced at a later date, the eleventh century to be precise. The introduction of this ware was a revolution from a technical point of view. The body was now composed of kaolin, flint in the form of sand, and feldspar, used instead of natural clay, and the glaze was chemically different from what it was before. It was now an alkaline glaze, instead of one that had a preponderent quantity of lead in the ingredients. The result of this change in the composition of the glaze was quite startling, for, from the copper oxide which had regularly been employed to get a brilliant green, the potters now got a brilliant blue, a color entirely absent in the earlier wares. Vessels of all kinds and amulets of this new color were made in great quantity. This major change passed entirely unnoticed in early literature and, from a casual examination of the pottery, could have easily been considered a passing whim of fashion. A technical examination showed very clearly, however, that the change was not one of mere chance. Desire and achievement are often far apart, but technique and fashion, like the words themselves, are more closely related than most people remember them to be.

One mold used in the making of this ware presents a rather charming scene of some musicians. It is of the Saljuk period of the eleventh or twelfth century. We had already found some actual bone pipes in a tenth-century house, but the pipes represented in the mold are of a different nature, much closer to the *zammara*, which is now played in Egypt It is much like a clarinet in shape but has no keys. One of the instruments depicted here is a lute. (The word is a corruption of the arabic *el 'Ood.*) Music in early Muslim times was not just a repetition of traditional folk songs There was a great deal of work being done on the theory of music and there were also many changes being made in the actual instruments themselves—particularly the lute.

At the risk of emphasizing a matter which may seem to be of little importance, I shall draw attention to another pastime, chess, practiced in Nishapur from the very beginning of the Islamic era. A number of ninth-century ivory chessmen were found in our excavations, as early as any set of pieces known. The invention of the game was credited by the Persians in the *Shahnameh* to the Hindus in the reign of Anurshirwan the Just, who reigned in the sixth century A.D. Through Persia, the game was introduced into Europe by the Arabs, though it was not widespread there until the eleventh century. In Spain a number of fine crystal pieces were preserved in various monasteries, and a cut-crystal set now in the collection of the Comtesse de Behagues shows a very close relationship to the pieces we found in Nishapur. In the manuscripts of Alfonso the Wise, written near the end of the thirteenth century, are a number of chess problems which have accompanying pictures. One is of two Arabs playing and another shows an Arab playing a Spaniard. In Nishapur, too, men of different races must have played many a game together.

I have been able only to give a glimpse or two of the way of life in early Nishapur, of its houses and the people who lived within, of their beliefs, their crafts, and their pastimes. Our excavations have not always borne out that which was written by the early writers, and occasionally only a study of modern manners and customs has enabled us to understand some of the things that we have found. We have also learned something of matters never written of at all. In an endeavor to explain a major change in fashion, we were indebted to simple chemical analyses and found out why this change took place. I have undoubtedly dealt with minor problems and touched on the things that have interested me personally, often matters of no importance whatever; yet perhaps I have also shown that some of the elements of life in that city and some of the niceties of their way of life can be resurrected and can prove to be not so strangely different from our own as we might have suspected. Much of our learning, whether we are conscious of it or not, has been derived from the same sources as these early people of Nishapur. Further, they themselves have added, to no small extent, to our own sum of knowledge.

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Elected November 17, 1949

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Rilett, Robert O., Ph.D., Entomology, Aquatic Biology. Associate Professor of Biology, University of Buffalo, Buffalo, N. Y.

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ADDITIONS TO LIFE MEMBERSHIP CLASS BY TRANSFER

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TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY[*]

PERMIAN FAUNAS OF THE GLASS MOUNTAINS, TEXAS, AND THEIR ENVIRONMENT

By G. ARTHUR COOPER[†]

The fossils which form the subject of this paper were prepared in the paleontological laboratory of the U. S. National Museum by etching blocks of Permian limestone taken from several formations in the Glass Mountains, west Texas. The Glass Mountains are located north of the village of Marathon. They extend in a northeast-southwest direction for 40 miles and straddle the Brewster-Pecos county line. The mountains are almost wholly composed of Permian limestones, and a nearly complete sequence is exposed.

The fossils come from only the lower three formations: Wolfcamp, Leonard, and Word. The formations above these are mostly dolomites or without silicified fossils. The Wolfcamp formation, 700 feet thick, consists mainly of shale, with thin or thick limestone lenses, the latter containing the bizarre fauna that has made the formation famous. The Leonard formation is about 2000 feet thick and is composed of two contrasting facies separated by reef limestones. The eastern facies is composed of fusulinid-bearing, thin-bedded limestones, while the western facies is composed of silicious limestones and shales. The upper 300 feet contain reefy deposits and abound in fossils. The Word formation is a great wedge, about 1500 feet thick, of silicious shales and thin limestones thinning to the northeast to 500 feet of cherty limestone. Four thick limestones which contain numerous fossils occur in the shaly mass on the thick side of the wedge.

All evidence points toward the fact that the environment of the Glass Mountains formations just noted was deposited in a near-shore zone. This fact can also be detected in the type of fossils and the sediments. Most of the limestones are very sandy and some contain quartz pebbles over an inch in diameter. Many of the fossils are thick-shelled brachiopods that formed clusters and small reefs in turbulent water.

The Permian fauna of the Glass Mountains abounds in all types of invertebrate animals. The fossils thus far brought together are the result of dissolving approximately 17 tons of limestone in dilute hydrochloric acid. This type of preparation of silicified fossils is a well-known and time-honored method of preparing specimens, but, at the National Museum, a new note was added to the familiar methods. The novelty is large scale. Blocks up to 200 pounds are processed and it is from these big lumps that the finest and largest specimens are obtained. The yield consists of many types of sponges, numerous small corals (few large and colonial corals are yet known from the Texas Permian), exquisite small crinoids, numerous molluscs (including spiny cephalopods), and a few trilobites. Of all the fossils obtained, the brachiopods are the most numerous and in greatest variety. They are of especial interest because of the high specialization obtained by some of them.

Most of the brachiopod superfamilies are present in the Glass Mountains fauna. The usual and familiar rhynchonellids, terebratulids, and spiriferids of the upper Paleozoic are abundant, but the most remarkable groups are the Orthotetinae, Leptodinae, and Productacea. Of the former groups, large *Derbyias* are common. Some are broad and flat, whereas others have long attenuated beaks. A variety of *Meekella* is common, and streptorhynchids of bizarre form are present. Of especial interest is the genus *Scacchinella*, which attains a length of over six inches and is one of the largest known brachiopods. This peculiar shell is suggestive of the rudistid pelecypods of the Cretaceous seas. The brachial valve is a lid to a deep conical valve. This genus has a huge forked cardinal process and the conical pedicle valve is filled with blister-like cysts. It lived in large clusters, with the valves closely cemented together.

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[*] This paper, illustrated with lantern slides, was presented at the meeting of the Section, December 5, 1949.

[†] United States National Museum, Washington, D. C.

Perhaps the most peculiar and most specialized brachiopod is *Leptodus*. This animal had an ostreiform shell, usually cemented in small clusters by the beak. The inner surface of the pedicle or attached valve has a median shallow groove from which extend lateral grooves nearly to the lateral margins. The brachial valve consists of a central axis and a series of unconnected ladder-like lobes on each side. The central axis of the brachial valve fits over the central groove, and the lateral lobes fit snugly into the lateral grooves of the pedicle valve. The shell is thus essentially a sieve, and the animal feeds by taking in food-bearing water currents in the spaces between the lateral lobes.

The Productacea is composed of several families of very spiny brachiopods. The spines have often been the subject of some speculation as to how they served the animal. Evidence from the Glass Mountains specimens shows that some of the productids were attached throughout life by their spines, others were attached during a short youthful stage but then broke off their attachment to live loose on the sea-bottom, and a third group was prevented from burial in the mud by its spines.

Two examples of permanently attached brachiopods may be mentioned. *Aulosteges* lived upright on some hard surface, with the anterior margin free but the posterior cemented down by a great brush of spines located along the posterior margin and on the ears. *Prorichthofenia* is another attached brachiopod which took the form of a cup-coral. The pedicle valve is conical, while the brachial valve is a lid-like operculum located below the mouth of the cup. The animal is cemented by long spines and the tip of the brachial valve to some solid object. In upper Leonard time it made clusters and small reefs on the sea floor.

Institella is a productid that was attached in young stages but broke free in later life. It often attached to crinoid stems but, as it grew, great spines extended posteriorly from the cardinal extremities for approximately two and one-half inches. In its adult form, it must have lain on its pedicle valve with the long cardinal spines stretched out and preventing it from being tipped over by currents. Another brachipod attached itself to seaweed or other small cylindrical objects by two spines that made a ring of attachment. After the shell became an adult and lived loose on the bottom, it was provided with a brush of long spines on each ear and long spines that curved over the anterior shell margin. No matter how the specimen came to rest on the sea bottom, its spines protected the front margin from being buried. It is believed the shell was moved about like a tumbleweed and may have been drifted with others into spongy masses of productids.

Shells that lived loose on the bottom, such as *Krotovia*, were provided with enormous cardinal spines. Another shell, *Yakovlevia*, had only four spines, two at the cardinal extremities and two extending horizontally from the place of geniculation of the front margin. The spines often attained a length of more than two inches and thus served to steady the shell on the mud as it lay convex side down.

All of these highly specialized creatures disappeared at the end of the Permian. They must have been a prey to their own unchangeability in the oscillating conditions of Permian time.

SECTION OF PHYSICS AND CHEMISTRY[*]

STABILITY STUDIES OF STRUCTURAL FRAMES[†]

By ABRAHAM SLAVIN[‡]

Synopsis. The purpose and scope of this investigation is: To determine, by stability studies of several typical rigid planar trusses of structural steel, the critical values and factors in terms of multiple loading, critical compressive unit stresses, relation of critical compressive unit stresses to yield point, critical values of the effective modulus of elasticity, and the column end restraint coefficients, and these on the basis of several design formulas, several concepts of the effective modulus of elasticity, and several yield point values.

To ascertain if simple and satisfactory approximations to the critical stability of several of the trusses may be obtained by analysis of small groups of the truss members and modifications of these groups rather than the truss as a whole.

Introduction. A number of papers have been written on the stability of plane frameworks. In particular, related column research has received noted attention. Stability analysis has received particular attention in the field of aeronautical engineering for structural frames, designed by ultimate load formulas, which have compression members in the long column range that fail at unit stresses considerably below the yield point. A search of the literature does not reveal a published study of the critical loading of typical, civil engineering, rigid planar trusses nor a comprehensive study of the use of groups of truss members for approximating the critical load of either airplane or civil engineering trusses.

The analyses for the civil engineering trusses of structural carbon steel show the futility of stability computations and indicate the use of the yield point as the practical critical compressive unit stress for these structures designed by working load formulas. For the included airplane and the typical civil engineering trusses, the possibilities of approximating the critical load from simplified assumptions of analysis of small groups of truss members and of the simplest group patterns, for brevity in computations to obtain satisfactory approximations, are indicated.

Theory and Method of Stability Analysis Used. Lundquist[1, 2] used the principles of the Cross[3] method of moment distribution to provide a method for determining the stability of structural members under axial load. The stiffness of the member depends upon the restraint condition at the far end. Three types of restraint are designated as: S, far end fixed; S′, far end elastically restrained; and S″, far end pinned. The stiffness of a member with constant cross section and fixed at the far end, is EI/L and $\frac{1}{4}$ of that by the Cross method. Three designations are used for the carry-over factor: C, far end fixed; C′, far end elastically restrained; and C″, far end pinned. It is assumed that the members lie in the plane of buckling. Two criteria for stability are indicated, the stiffness criterion and the series criterion, either of which may be used. Both the stiffness and the carry-over factor are functions of the axial load in the member. From the moment distribution for the applied unit moment in FIGURE 1, the stiffness criterion for stability is:

$$\Sigma S'bc > 0. \tag{1}$$

For neutral stability, which gives the critical buckling load for the structure, the stiffness stability factor is set equal to zero, or:

$$\Sigma S'bc = 0. \tag{2}$$

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[*] This paper, illustrated with lantern slides, was presented at the meeting of the Section, December 6, 1949. It is an abridgment of the paper which was awarded an A. Cressy Morrison Prize in Natural Science in 1949 by the New York Academy of Sciences (*cf*. p. 112).

[†] An abridgment of the thesis accepted by the faculty of the Graduate Division of the College of Engineering of New York University in partial fulfillment of the requirements for the degree of Doctor of Engineering Science.

[‡] Consulting engineer and architect, New York, N. Y.; Department of Civil Engineering, Polytechnic Institute of Brooklyn, Brooklyn, N. Y.

Since there is more than one critical buckling load, the satisfaction of equation (1) is insufficient. The lowest load that satisfies equation (2) is the critical load to be considered.

The series criterion for stability is derived by applying an external unit moment at joint b of the section of a structure, as in FIGURE 2. Far ends of the members are assumed as elastically restrained by coiled springs to allow for the other members beyond the joints at a_1, a_2, a_3, and d_1, d_2, and d_3. In the Cross method, the moment at the end of any member is some quantity times the infinite series $(1 + r + r^2 + r^3 \cdots)$ or $(1/1-r)$. From the moment distribution analysis,[1,2] the total moment in members ba at joint b, is:

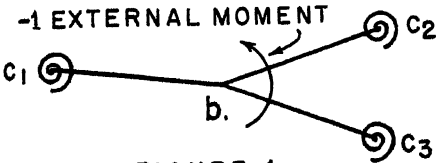


FIGURE 1

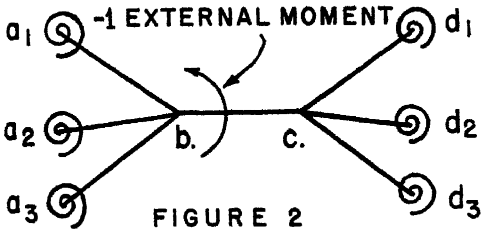


FIGURE 2

$$\frac{S'ba}{Sbc + \Sigma S'ba}\,\frac{1}{1 - r} \qquad (3)$$

where

$$r = \frac{Sbc\ Cbc}{Sbc + \Sigma S'ba}\,\frac{Scb\ Ccb}{Scb + \Sigma S'cd}. \qquad (4)$$

Equation (4) represents the series stability factor or unbalanced moment at joint b. Since the total moment in each member must be finite for stability, the series criterion for stability is:

$$0 < r < 1. \qquad (5)$$

For neutral stability, which gives the critical buckling load of the structure, the series stability factor or unbalanced moment is set equal to unity, or:

$$r = 1. \qquad (6)$$

As noted for the stiffness criterion, the lowest load that satisfies equation (6) is the critical load.

If the carry-over factor is omitted from each part of equation (4), we have the stiffness of the particular member divided by the summation of the stiffness of all the members at the joint, or what is termed $K/\Sigma K$ in moment distribution. This ratio is herein indicated as S_rbc, S_rcd, *etc.*, and called the stiffness ratio. Accordingly, equations (3) and (4) are expressed as:

$$\frac{S'_rba}{1 - r} \qquad (7)$$

and

$$r = (S_rbc\ Cbc)\ (S_rcb\ Ccb). \qquad (8)$$

For symmetrical conditions, with $S_rbc = S_rcb$, and $Cbc = Ccb$,

$$r = (S_rbc\ Cbc).^2 \qquad (9)$$

Although the method proposed is indicated for analysis of groups of members of a truss for approximations to the critical stability, its use is also applicable to an entire truss.

James[6] combined the formulas originally derived by Muller-Breslau in about 1902 and extended by Niles and Newell[4,5] with the Cross procedure to allow for beam-column action in rigid frame analysis. With the inclusion of axial load, either tension or compression, in the members of a frame, the moment distribution is modified to the extent that the distribution factors, carry-over factors, and fixed-end moments become functions of L/j instead of being constant for a given number. The L/j value is an abstract number and is computed from the equation of $L/j = L\sqrt{P/EI}$. The moment-distribution coefficients to allow for axial load in compression or tension, in terms of L/j, are given by Niles and Newell,[5] James,[6] and Lundquist and Kroll.[7, 8]

Hoff[9, 10] developed a method for ascertaining the stability of any system of axial loaded members, applicable to rigid jointed trusses and continuous beam-columns, to which may be applied the Cross method of moment distribution as modified for axial load by James. For simplification in the computations, fixed ends are assumed for joints elastically restrained. For verification of the equivalence of the critical load, where "r" equals unity, the unit moment should be applied separately to each joint at which compression members enter. To satisfy the requirements, "r" must be unique and finite. The moment distribution procedure of Niles and Newell[5] is used.

The design load is notated as Load Factor 1.00, with other load factors corresponding to the respective multiple load values. If the summation of the stiffness of all the members at any joint becomes negative, then the critical load has been exceeded. In the absence of definite criteria to determine the critical load factor of the particular type of structural frame considered, it becomes necessary to evaluate "r" for a series of load factors. Curves of "r" versus load factor are plotted for each joint investigated. The first crossing of such curves with the line of "r" = 1.00 indicates the critical load factor, although above and below the critical load factor the curves may not coincide, due to the varying stiffness of the joints. If the truss, in which the members are interdependent, is loaded to its critical limit, then the entire truss must be in a condition of neutral equilibrium and the "r" versus load factor curves for the several joints will cross the line of "r" = 1.00 at the same critical load factor. However, the critical load factor for a group of members of the truss may be either above or below the true value determined from analysis of the entire frame.

Equilibrium Conditions. From the fundamental theory of stability of columns, it is known that if the centrally applied compressive force is less than the critical value, the column will remain straight. This straight condition of equilibrium indicates that the column is stable. With a slight increase in the compressive load above its critical value, the column will buckle.

To illustrate the equilibrium conditions, three cases of equilibrium of a ball are shown in FIGURE 3. It is evident that the ball on the concave spherical surface (a) is in a state of stable equilibrium, and that the ball on the convex spherical surface (c) is in a condition of unstable equilibrium, while, for the horizontal surface (b), the ball is in a state of neutral equilibrium. Considering the energy of the system, the previous conclusions can be made. In FIGURE 3 (a), any displacement of the ball will raise the center of gravity of the ball, which will require a certain amount of work that will increase the potential energy of the system. In FIGURE 3 (c), any displacement from the position of equilibrium will lower the center of gravity of the ball which will decrease the potential energy of the system. For the ball on the horizontal surface, a condition of neutral equilibrium, any displacement will not change the energy of the system. For the state of stable equilibrium, the energy of the system is a minimum, while, for the condition of unstable equilibrium, the energy of the system is a maximum. A column may be in a stable or unstable equilibrium condition depending on the value of the compressive load in relation to its critical value. The compressive load attains its critical value when the equilibrium condition changes from the stable to the unstable state.

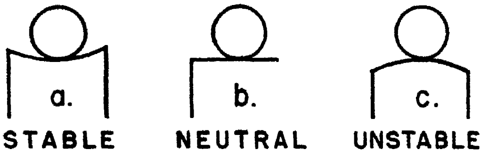


FIG.3. EQUILIBRIUM CONDITIONS

Hoff applied the same principles of least energy, called "the Maupertuis' theorem," to the investigation of the stability of plane frameworks. He demonstrated that, if the framework is stable, the total potential energy of the entire framework is a minimum and that, by the Cross procedure, the values of the end moments corresponding to this minimum can be approximated by a sufficient number of steps of moment distribution, in each of which the total potential energy decreases. When $L/j < \pi$, the potential energy is a minimum and the equilibrium condition is stable. For a compression member with pinned ends and with $L/j > \pi$, the value of the total potential energy corresponds to a saddle point of the surface, with resulting unstable equilibrium.

Groups of Members. The use of groups of members of a frame for approximations in stability analysis has been indicated in the literature on stability research. The group receiving most attention for aeronautical frames consists of a compression member with its adjacent members having terminals, once removed, that are pin-ended. The assumption of pinned terminals results in conservative values for the critical load of the group. It is evident that judgment is required in selecting the members to form a group for approximation analysis. Tension members at the ends of a buckling member will increase the critical load. Adjacent compression members that are highly stressed and have pinned terminals will be of little value, if any, in resisting the buckling of the prime member under consideration. Pinned terminals at the adjacent members reduce their stiffness, while fixed terminals increase their stiffness. The greater the stiffness of the adjacent members, the greater the resistance offered to the buckling member. Members unstressed or having a reserve of buckling strength are satisfactory adjacent members in the sense of offering resistance to the buckling of the prime compression member.

In the included studies of the several typical trusses, the following groups of members and variations of same are indicated for analysis to ascertain the approximate critical load. (a) A buckling member, with adjacent members having terminals once removed, which is one joint from the end of the compression member. The adjacent members are to have terminals that are either pinned, fixed for compression members and pinned for tension members, all fixed, or fixed for compression members with the tension members omitted. A unit moment, to be applied at either end of the buckling member, will give the same "r." (b) A buckling member with adjacent members continued for terminals two joints from the end of the compression member. The terminals of the extended adjacent members are to be either all pinned or all fixed, with a variation in the pin-ended group of the omission of some of the compression members beyond one joint from the end of the buckling member. A unit moment is to be applied separately at each end of the prime buckling member. The "r" will not be the same for analysis at each end of the buckling member except where the critical load is indicated. (c) A joint having compression members, with terminals two joints removed, that are either all pinned, fixed for compression members and pin-ended for tension members, or all fixed, with a variation in the pin-ended group of the omission of some of the compression members beyond one joint removed from the selected joint. A unit moment is applied at the particular joint. The number of groups of members will accordingly depend on the number of buckling members, the number of joints having compression members, the variations due to terminal restraints, the omission of adjacent buckling members that reduce the critical load factor, and the omission of tension members where indicated. The approximations will be further affected by the design method for the structure, since airplane trusses are designed by ultimate load formulas while civil engineering trusses are designed by working load formulas. At the design load, the former, which have slender compression members, have high L/j values, while the latter, with stocky compression members, have low L/j values. In airplane trusses, the compression and tension members may be of sections that are practically equal, while in civil engineering trusses the tension members are light compared to the stocky compression members. These differences in sections affect the stiffness ratios of the members at the joints and in turn the value of the critical load.

Approximate Estimate of Critical Load. Lundquist[2, 11] indicated a method of estimating the lowest critical buckling load for structural members by using the relations between the load on the structure and rotations of a joint after his equations for neutral stability[1, 2] have been tested for three assumed critical loads, each of which assumed loads is less than the lowest critical load on the basis of the axial load in each compression member being below that of its strength as a pin-ended strut.

The studies of several of the included civil engineering trusses, on basis of E_u, indicate that an estimate of the critical load may be approximated from the range in load factors referenced to the corresponding values of L/j between π and 2π for the prime compression member, which is the buckling member having the highest L/j at the critical load. When such range in load factors is within practical tolerances, the analysis for the critical load may be omitted. This approximation is not applicable to airplane trusses having slender members.

Effective Modulus of Elasticity. When a column is subject to stress above the proportional limit, the modulus values will in general become reduced. Therefore, the resistance of the section to bending deformation will not be measured by EI, as for the case where the stresses are below the proportional limit, but by $\bar{E}I$, where $\bar{E}$ is the effective modulus of elasticity for the section under the given stress distribution. Investigators have shown that the value of E depends on the intensities and distribution of the stresses, the corresponding values of the tangent modulus E_t, and the shape of the cross section. E_d, the double modulus, will always be between the values of E and E_t.

Borkman[12] suggests an expedient estimate of the effective modulus on the basis of a linear relation between the proportional limit and the yield point. Osgood[13] gives a summary of the origin of the double-modulus theory of column action and a brief of its mathematical basis.

The ultimate-load, short column design formulas in the Niles and Newell text[4] are used as a basis for a practical determination of the value of $\bar{E}$. This reduced modulus, E_u, is of a lower value than E_t and is intended to replace the Engesser and Karman formulas, which make no allowance for the effect of deviation of the practical column from the corresponding ideal column. This method of computing E, which is backfigured from the empirical short column formulas, is a purely empirical engineering method of getting a value for practical use and, while it may not be justified from a theoretical consideration, it has been found that the practice gives reasonable results. For a low value of $\bar{E}$, the L/j is increased with a corresponding conservative critical load. This method is also given by the Army-Navy-Civil Committee on Aircraft Design Criteria.[14] They remark that the modified Euler formula does not have much practical importance in determining the short-column curve, but it is of practical interest in connection with the determination of the effective modulus which can be used to compute instability stresses. The value of this reduced modulus, E_u, at any given compressive stress is determined by solving the modified Euler equation after substituting the critical compressive unit stress and the corresponding slenderness ratio as obtained from the basic column formula. The equations express $E_u = E$ at one half the yield point, with a decrease in E_u to zero value at the yield point.

The assumption of computing $\bar{E}$ for tension stress on the same basis used for compression stress offers a convenient solution. An exception is made in this investigation to the assumption which requires $\bar{E} = 0$ when the tension stress is greater than the maximum allowed for a column of the same material. Since tension stresses in excess of yield point do not necessarily indicate failure, the value of E in tension members of structural carbon steel that are stressed above the yield point, under loading that is not critical for the compression members of the structural frame, is computed on the basis of the linear relation of a portion of the idealized stress-strain curve above the yield point.

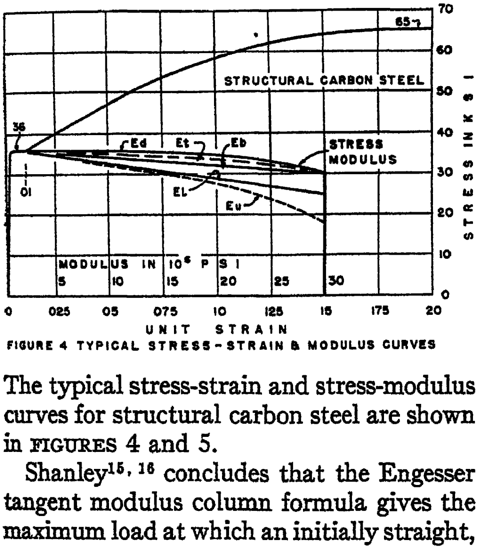


FIGURE 4 TYPICAL STRESS-STRAIN & MODULUS CURVES

The typical stress-strain and stress-modulus curves for structural carbon steel are shown in FIGURES 4 and 5.

Shanley[15, 16] concludes that the Engesser tangent modulus column formula gives the maximum load at which an initially straight, centrally loaded column remains straight, that this equation should be used as a basis for determining the buckling strength of members in the inelastic range, and that loading beyond the tangent modulus will produce permanent bending deformation. This recommendation for the tangent modulus verifies the prior conclusions by Templin, Hartman, Sturm, Holt, and Paul[17, 18] on the basis of tests of ideal columns of aluminum alloys, hot rolled steel of properties very close to structural carbon steel, magnesium alloys, and annealed steel sheet.

E_L is the author's suggested linear effective modulus, being a linear relation between 0.7 yield point and the yield point. This is a purely empirical relation for the purpose of simplification in computation of the effective modulus. It is intended to include an allowance for the deviation between the ideal and corresponding practical column and to give weight to the proportional limit stress above half the yield point. For metals other than structural steel, which may have a proportional limit below half the yield point, the suggested linear modulus is not intended to apply.*

Column Formulas. The relation between the Euler formula and the corresponding values of L/j which depend on axial load follow. For a column with pinned ends:

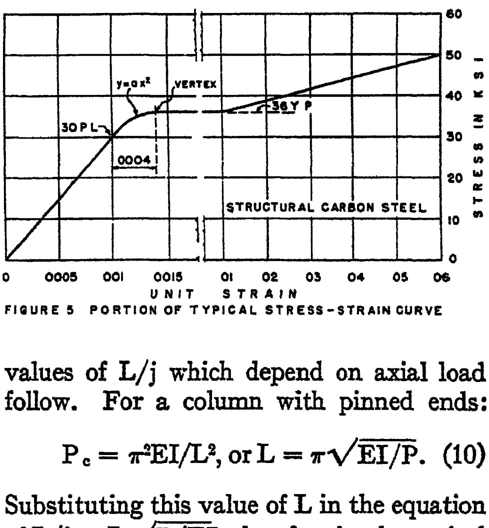


FIGURE 5 PORTION OF TYPICAL STRESS-STRAIN CURVE

$$P_c = \pi^2 EI/L^2, \text{ or } L = \pi\sqrt{EI/P}. \quad (10)$$

Substituting this value of L in the equation of $L/j = L\sqrt{P/EI}$, then for the theoretical pinned-end column

$$L/j = \pi. \quad (11)$$

For the restrained-end column,

$$P_c = c\pi^2 EI/L^2, \text{ and } L = \pi\sqrt{cEI/P} \quad (12)$$

and

$$L/j = \pi\sqrt{c} = \pi\sqrt{1/k^2} = \pi/k. \quad (13)$$

---

* Tabulated values of E_t, E_u, E_d, E_L, E_b and L/r modulus curves for structural carbon steel and reference to the evaluation of the effective modulus by investigators not mentioned herein are given in the dissertation

For the theoretical fixed-end column, $c = 4.0$ and

$$L/j = 2\pi. \tag{14}$$

The ultimate load column formula for the airplane truss, with a yield point of 36,000 p.s.i. and E of 28,000,000 p.s.i., in terms of allowable design stress in p.s.i.,[14] is:

$$36{,}000 - \frac{1.172}{c}(L/r)^2. \tag{15}$$

For theoretical restrained ends, as commonly used in airplane frame design, $c = 2.0$.

Three working load column formulas, in terms of allowable working stress in design in p.s.i., are used for the structural carbon steel trusses:*

Old A.R.E.A.: $16{,}000 - 70\ (L/r)$; (16)

A.I.S.C., for L/r not greater than 120:

$$17{,}000 - 0.485\ (L/r)^2 \tag{17}$$

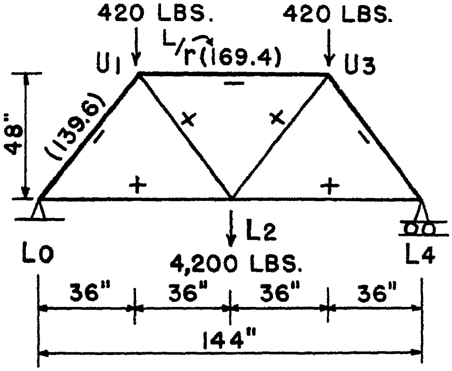
(although coefficients for different end conditions are not indicated in these two formulas, they are intended to apply to the practical column in construction); the 1943 A.R.E.A. formula for bridges, for L/r not greater than 140, and for riveted ends: $15{,}000 - (1/4)(L/r)^2$. (18)

Stability Analysis of Structural Frames

Airplane Truss (*2 Studies*). The truss, designed by ultimate load formulas, is taken from Niles and Newell[5] and is composed of carbon steel tubular section members having slenderness ratios in the long column range. The first study is based on E_u, which is the general practice in aircraft-frame analysis, and the second study on the basis of E. Since E_u at the design load factor 1.00 is close to E for all members of the truss, the second study is included to ascertain if the increase in critical load on basis of E is within a practical tolerance of the critical load on the modulus concept of E_u. E_u is calculated from:

$$\bar{E} = \frac{P}{A}\,\frac{[36{,}000 - (P/A)]}{1.172\pi^2}. \tag{19}$$

Study No. 1. Yield point at 36,000 p.s.i., proportional limit at 30,000 p.s.i., modulus concept is E_u, and E at 28,000,000 p.s.i. as commonly used for steel tubing. Basic data for the design load factor 1.00 is shown in FIGURE 6 and TABLE 1. Gross areas are used for computing the unit stress. $L/j = L\sqrt{P/\bar{E}I}$; C.O., the carry-over factor, and $SL/\bar{E}I$, the stiffness coefficient for respective values of L/j in axial compression or tension, are interpolated from tabular values; EI/L is computed; S, the stiffness of member, is the product of SL/EI by EI/L; stiffness of joint is the sum of the stiffness of each member entering the joint. Since the L/j at the design load of all the truss members are above π but are less than 2π, the compression members would be unstable if they were pin connected. For joints L_0 and U_1 to have zero stiffness on the basis of stiffness coefficients for far ends fixed, the loading may be increased about seventeen per cent above the design load.



AIRPLANE TRUSS – STUDY No 1
FIG. 6. DESIGN LOAD FACTOR 1.00

As the summation of the stiffness for all the members meeting at joints L_0, U_1, and L_2 is positive for the design load, the stiffness criterion, which is a necessary condition for stability, is satisfied. However, this is not a complete proof that the truss is stable. A unit moment is applied separately at joints L_0 and U_1, and distributing this applied moment will indicate whether or not the unbalanced moment "r" is less than unity. To ascertain if "r" is unique and finite, the unit moment is distributed in a different order of balancing the joints to determine if essentially the same value of the unbalanced moment is obtained. If the "r" is less than unity, the series criterion for stability is satisfied, the series is convergent, and the truss is stable under the respective loading. The "r" curve is shown in FIGURE 7.

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* Tabulated values for design stresses and the column curves are given in the dissertation.

TABLE 1

AIRPLANE TRUSS-STUDY NO. 1—DESIGN LOAD FACTOR 1.00

Member	*Size inches*	*Stress pounds*	*Area sq in*	*Stress p s i*	$\frac{Eu}{1,000}$	*Length inches*	*Mom. of inertia I*	$\frac{L}{j}$
L_0 U_1 U_3 L_4	$1\frac{1}{4}$ × 0.035	−3,150	0.1336	−23,577	25,321	60	0.02467	4.261
U_1 L_2 L_2 U_3	$1\frac{1}{4}$ × 0.035	+2,625	0.1336	+19,648	27,775	60	0.02467	3.714
U_1 U_3	$1\frac{1}{4}$ × 0.049	−3,465	0.1849	−18,740	27,963	72	0 03339	4 386
L_0 L_2 L_2 L_4	$1\frac{1}{4}$ × 0.035	+1,890	0.1336	+14,147	28,000	72	0 02467	3 766

Member	$\frac{L}{j}$	$\frac{SL}{EuI}$	$\frac{EuI}{L}$	*S*	*Σs, L_0*	*Σs, U_1*	*Σs, L_2*	*C. O.*
L_0 U_1 U_3 L_4	4.261	0.1518	10,411	1,580	1,580	1,580	—	5.607
U_1 L_2 L_2 U_3	3.714	1.3955	11,420	15,937	—	15,937	$\frac{15,937}{15,937}$	0.3012
U_1 U_3	4.386	0.0741	12,968	961	—	961	—	11.642
L_0 L_2 L_2 L_4	3.766	1.4048	9,594	13,478	13,478	—	$\frac{13,478}{13,478}$	0 2981
				Σ	15,058	18,478	58,830	—

Stress +: tension
−: compression

Analyses of groups of members of the airplane truss were made to determine if simple and satisfactory approximations to the critical stability may be obtained by analysis of small groups of the truss members and modifications of such groups rather than the truss as a whole. Six groups were considered: (1)—L_0 U_1 as the prime compression member with terminals once removed from its ends; (2)—same as for 1 except U_1 U_3 is the prime compression member; (3)—L_0 U_1 as the prime compression member with terminals twice removed from its ends; (4)—same as 3 except U_1 U_3 is the prime compression member; (5)—unit moment at L_0 with terminals twice removed; (6)—unit moment at U_1 with terminals twice removed. For (2), (3), and (4), the unit moment is applied separately at each end of the prime member. Compression members are either omitted or have fixed terminals in the fifth and sixth groups, where it is known from the basic data and analysis of prior groups that such members contribute to instability at low load factors. Tension members are omitted in some of the groups to determine their effect on the value of the critical load.

Instability of a group of members of a truss does not necessarily indicate that the truss as a whole is unstable for the same loading. The critical load of groups of members will be either below or above or practically at the true critical load on the basis of the entire truss. The simplest group of members that will give a critical load in close approximation to the true value is desired. Results are discussed in the "Summary and Conclusion."

Study No. 2. Yield point, proportional limit, and basic design are the same as in Study No. 1, but the full value of E of 28,000,000 p.s.i. is used for all loadings. The critical load factor on the basis of E is 1.0464 and is 1.50 per cent above the value of 1.0310 on basis of E_u. It was found that for the tension members the reduction in L/j with the change from E_u to E is insignificant. Member $U_1 U_3$ is the prime compression member in the entire truss, although member $L_0 U_1$ has the highest compressive stress at the true critical load.

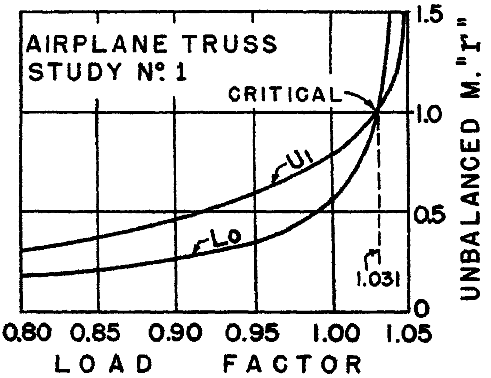
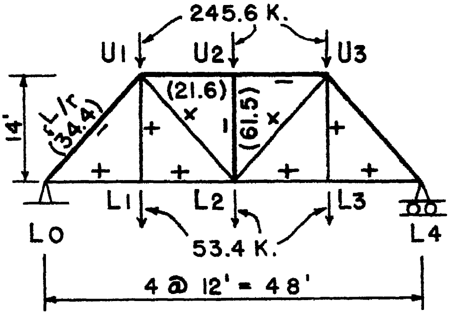


FIGURE 7

4 Panel Building Truss (9 Studies). This civil engineering truss of structural carbon steel, with compression members in the short column range, is designed by working load formulas. Design loading and E of 30,000,000 p.s.i. are the same for all the studies. Proportional limit, yield point, and $\bar{E}$, are varied, as indicated. The first seven studies are of the Warren type truss, with the eighth and ninth studies modified for a Howe type truss.

Study No. 1. Yield point at 36,000 p.s.i., proportional limit at 30,000 p.s.i., modulus concept is E_u, and the Old A.R.E.A. design specifications. Truss diagram and loading are shown in FIGURE 8.*

The L/j, C.O., SL/EI, and S, are computed as noted for the airplane truss. Gross areas are used throughout. With the exception of tension member $L_1 U_1$, which has an L/j at the design load of slightly above π, the L/j of all the other members are less than π. The compression members would, therefore, be stable at the design load if they were pin-connected. On basis of working load formulas, at the design load the numerical values of L, j are normally low for the compression members. Computed joint stiffness, on the basis of far end fixed, indicates positive values for all the joints up to the load factor 2.5640. A unit moment is applied separately at the joints which have compression members to determine the "r" for each loading increment. The prime compression member is $U_1 U_2$, which at the true critical load factor of 2.5553 has the highest L/j, highest compressive unit stress, and the lowest value of E_u.



4 PANEL BLDG. TRUSS-STUDY No 1
FIG. 8. DESIGN LOAD FACTOR 1.00

Analyses of groups of members of the 4 panel building truss, Study No. 1, were made to determine if satisfactory approximations to the critical stability may be obtained. Eleven groups, with a total of 39 combinations, were considered. These groups and combinations are an extension of those used for the airplane truss and follow the same pattern as shown and discussed for the 6 panel truss. Of particular note is that the approximate critical load factors from the stability analysis of truss member groups are within 0.5 per cent of the true value. The omission of tension members, which are generally of light section in civil engineering trusses, have no appreciable effect on the critical load factor. FIGURE 9 shows a portion of the "r" *vs.* Load Factor curve for analysis of the entire truss.

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* Taken from a design by Professor E. J. Squire of the Polytechnic Institute of Brooklyn.

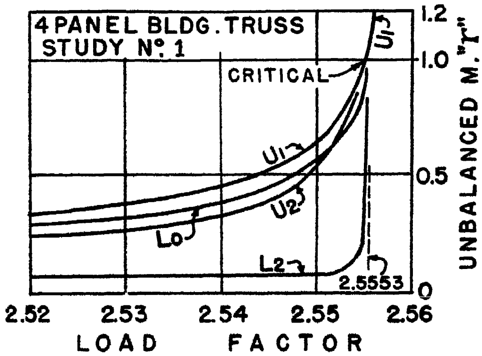

FIGURE 9

Studies No. 2, 3, and 4. In Study No. 2, the same design, proportional limit, and E_u are used as in Study No. 1, but the yield point is increased to 40,000 p.s.i. In Studies No. 3 and 4, the design is based on A.I.S.C. specifications, proportional limit at 30,000 p.s.i., and E_u. The yield point is 36,000 p.s.i. for Study No. 3 and 40,000 p.s.i. for Study No. 4. Positive stiffness for all the joints and L/j less than π for the compression members are indicated at the design load. The L/j and related values for Study No. 4 differ from those in Study No. 3 due to increase in the yield point.*

The prime compression member is U_1 U_2 in Studies No. 2 and 3 and L_0 U_1 in Study No. 4.

Studies No. 5, 6, and 7. These studies are based on E_t. In Study No. 5, design, proportional limit, and yield point are the same as for Study No. 1. Study No. 6 is based on the same design, proportional limit, and yield point as Study No. 3. Study No. 7 has the same design as Study No. 6, but the proportional limit is 27,000 p.s.i. and the yield point is 33,000 p.si., with the critical load factor determined from the analysis of two simple patterns of members of the truss of the first group as shown for the six panel truss.*

It is expected that the analysis of the group with fixed terminals will give a slightly higher critical load than the group with pinned terminals. The analysis indicates that either group gives practically the same critical load between the narrow limits of load factors 2.0270 and 2.0275. The prime compression member is U_1 U_2 in Study No. 5 and L_0 U_1 in Studies No. 6 and 7.

Studies No. 8 and 9. These studies for the Howe type truss are based on A.I.S.C. design specifications, E_u, and proportional limit of 30,000 p.s.i. Yield point is 36,000 p.s.i. for Study No. 8 and 40,000 p.s.i. for Study No. 9. The same truss dimensions and loading as shown for Studies No. 1 to 7 apply, except that member L_1 U_2 is parallel to member L_0 U_1. Tabulated values of "r" are given in the thesis. Member U_1 U_2 has the highest unit stress but L_0 U_1 has the highest L/j. With close to the same unit stresses in these two members, the higher L/j in L_0 U_1 is due mainly to its increased length. These two studies indicate that the member having the highest compressive unit stress may not have the critical L/j value.

Modulus Concept. The studies on the basis of E_t and on the basis of E_u indicate that the critical compressive unit stresses are practically at the yield point. Therefore, the studies were not extended to determine the critical compressive unit stresses on the basis of E_d, E_b, or E_L. An illustration of the effect of several different concepts of the effective modulus of elasticity on the critical values for the same design in Studies No. 1 and 5 is shown in FIGURE 10. At the critical load factor 2.5553 in Study No. 1, E_u concept, the L/j in U_1 U_2, the prime compression member, is 4.108 and the unit stress is 35,681 p.s.i. or about 300 p.s.i. below the yield point. For the same design, but with the modulus concepts of E_t and E_d, the critical load factor is close to 2.58, with the unit stress in U_1 U_2 practically at the yield point.

---

* Tabulated values of "r" vs. Load Factor at joint L_0, U_1, and U_2 for loading above and below the critical load, on the basis of analysis of the entire truss for Studies No. 1, 2, 3, and 4, are given in the dissertation.

* Tabulated values of "r" vs. Load Factor for loading below and above the critical load are given in the dissertation.

6 Panel Highway Truss (2 Studies). This civil engineering truss of structural carbon steel, with the highly stressed compression members in the short column range, is designed by the 1943 A.R.E.A. working load formulas. Design loading, specifications, and E of 30,000,000 p.s.i. are the same for both studies. Proportional limit, yield point, and E are varied as indicated. Member L_2 U_3 has a low design unit stress due to limiting value for L/r.

Study No. 1. Yield point at 36,000 p.s.i., proportional limit at 30,000 p.s.i., and modulus concept of E_u. At the design load the compression members would be stable if pin-connected. Computed joint stiffness, on the basis of far ends fixed, indicates positive values for all the joints up to and including the load factor 2.43 or in excess of the true critical load factor of 2.3983 for the entire truss. The prime compression member is U_1 U_2, which at the true critical load has the highest L/j and compressive unit stress and the lowest value of E_u in compression. Critical L/j is 3.707 in L_0 U_1 and 3.977 in U_1 U_2.

FIGURE 11 shows the truss diagram and loading and the all pinned-end and corresponding all fixed-end groups of members for approximation studies. The first to fourth groups include a compression member with terminals once removed from each end. The fifth to eighth groups include a compression member with terminals twice removed. The ninth to thirteenth groups have the terminals twice removed from the joint where the unit moment is applied. The approximate critical load factors obtained from analysis of all the groups considered are within a maximum variation of 2.50 per cent of the true critical load factor. An approximation within 1.0 per cent is noted for 12 of 26 patterns. One or two additional patterns under each group, for a total of 22 patterns not indicated on FIGURE 11, based on combinations of fixed and pinned ends, omission of highly stressed compression members, and omission of some of the tension members, were also analyzed, with a resulting variation from the true critical load factor within 2.50 per cent. It was found that the omission of tension members has no appreciable effect on the critical load factor. The per cent variation from the true critical load for each group is marked on FIGURE 11.

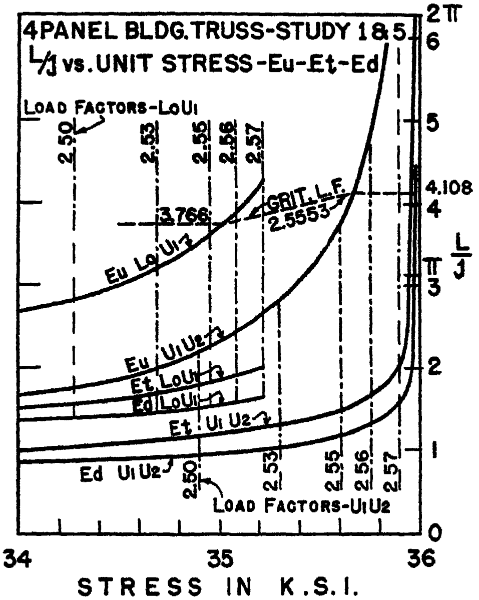

FIGURE 10

Study No. 2. Same design as in Study No. 1, but the proportional limit is reduced to 27,000 p.s.i. and the yield point to 33,000 p.s.i. and the modulus concept is E_t. The critical load factor is determined from

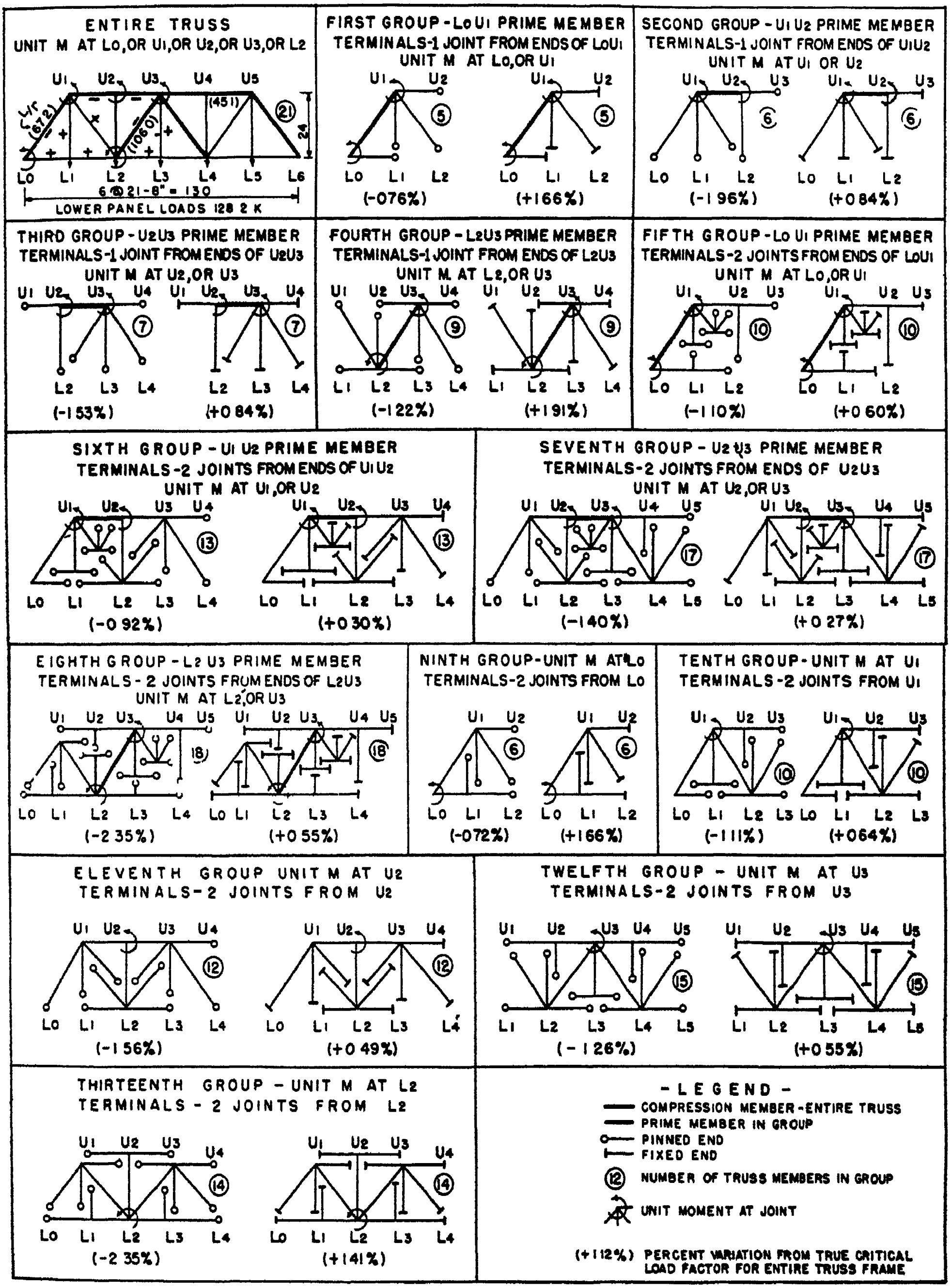
ENTIRE TRUSS
UNIT M AT Lo,OR U1,OR U2,OR U3,OR L2
U1 U2 U3 U4 U5
(672)
(1060)
(451)
21
24
Lo L1 L2 L3 L4 L5 L6
6 @ 21-8" = 130
LOWER PANEL LOADS 128 2 K
FIRST GROUP - LoU1 PRIME MEMBER
TERMINALS-1 JOINT FROM ENDS OF LoU1
UNIT M AT Lo,OR U1
U1 U2 U1 U2
5
Lo L1 L2 Lo L1 L2
(-076%) (+166%)
SECOND GROUP - U1 U2 PRIME MEMBER
TERMINALS-1 JOINT FROM ENDS OF U1U2
UNIT M AT U1 OR U2
U1 U2 U3 U1 U2 U3
6
Lo L1 L2 Lo L1 L2
(-1 96%) (+0 84%)
THIRD GROUP - U2U3 PRIME MEMBER
TERMINALS-1 JOINT FROM ENDS OF U2U3
UNIT M AT U2,OR U3
U1 U2 U3 U4 U1 U2 U3 U4
7
L2 L3 L4 L2 L3 L4
(-1 53%) (+0 84%)
FOURTH GROUP - L2U3 PRIME MEMBER
TERMINALS-1 JOINT FROM ENDS OF L2U3
UNIT M AT L2,OR U3
U1 U2 U3 U4 U1 U2 U3 U4
9
L1 L2 L3 L4 L1 L2 L3 L4
(-1 22%) (+1 91%)
FIFTH GROUP - Lo U1 PRIME MEMBER
TERMINALS-2 JOINTS FROM ENDS OF LoU1
UNIT M AT Lo,OR U1
U1 U2 U3 U1 U2 U3
10
Lo L1 L2 Lo L1 L2
(-1 10%) (+0 60%)
SIXTH GROUP - U1 U2 PRIME MEMBER
TERMINALS-2 JOINTS FROM ENDS OF U1U2
UNIT M AT U1,OR U2
U1 U2 U3 U4 U1 U2 U3 U4
13
Lo L1 L2 L3 L4 Lo L1 L2 L3 L4
(-0 92%) (+0 30%)
SEVENTH GROUP - U2U3 PRIME MEMBER
TERMINALS-2 JOINTS FROM ENDS OF U2U3
UNIT M AT U2,OR U3
U1 U2 U3 U4 U5 U1 U2 U3 U4 U5
17
Lo L1 L2 L3 L4 L5 Lo L1 L2 L3 L4 L5
(-1 40%) (+0 27%)
EIGHTH GROUP - L2 U3 PRIME MEMBER
TERMINALS - 2 JOINTS FROM ENDS OF L2U3
UNIT M AT L2,OR U3
U1 U2 U3 U4 U5 U1 U2 U3 U4 U5
18
Lo L1 L2 L3 L4 Lo L1 L2 L3 L4
(-2 35%) (+0 55%)
NINTH GROUP-UNIT M AT Lo
TERMINALS-2 JOINTS FROM Lo
U1 U2 U1 U2
6
Lo L1 L2 Lo L1 L2
(-072%) (+166%)
TENTH GROUP-UNIT M AT U1
TERMINALS -2 JOINTS FROM U1
U1 U2 U3 U1 U2 U3
10
Lo L1 L2 L3 Lo L1 L2 L3
(-1 11%) (+064%)
ELEVENTH GROUP UNIT M AT U2
TERMINALS-2 JOINTS FROM U2
U1 U2 U3 U4 U1 U2 U3 U4
12
Lo L1 L2 L3 L4 Lo L1 L2 L3 L4
(-1 56%) (+0 49%)
TWELFTH GROUP - UNIT M AT U3
TERMINALS-2 JOINTS FROM U3
U1 U2 U3 U4 U5 U1 U2 U3 U4 U5
15
L1 L2 L3 L4 L5 L1 L2 L3 L4 L5
(- 1 26%) (+0 55%)
THIRTEENTH GROUP - UNIT M AT L2
TERMINALS - 2 JOINTS FROM L2
U1 U2 U3 U4 U1 U2 U3 U4
14
Lo L1 L2 L3 L4 Lo L1 L2 L3 L4
(-2 35%) (+1 41%)
- LEGEND -
COMPRESSION MEMBER - ENTIRE TRUSS
PRIME MEMBER IN GROUP
PINNED END
FIXED END
12 NUMBER OF TRUSS MEMBERS IN GROUP
UNIT MOMENT AT JOINT
(+1 12%) PERCENT VARIATION FROM TRUE CRITICAL LOAD FACTOR FOR ENTIRE TRUSS FRAME

FIGURE 11 — 6 PANEL HIGHWAY TRUSS — GROUPS OF MEMBERS

the analysis of two patterns each of the second and sixth group on FIGURE 11. The computed values of the joint stiffness, on the basis of far end fixed, are all positive at the taken value of the critical load, with zero stiffness for the joint U_2 of least stiffness at 0.08 per cent above the critical load. Practically the same critical load factor is obtained from the analysis of either of the four groups, with a taken average of 2.2865.

Modulus Concepts. Since E_d values are slightly higher than E_t and since, on the basis of E_t, the critical compressive stress is close to the yield point, analysis on basis of E_d is omitted. The critical unit compressive stress on basis of E_L can be considered the same as on the concept of E_u, and for E_b it may be assumed as between the limits on basis of E_u and E_t.

Summary and Conclusion

Stability Analysis. Of the two stability criterions, the modified stiffness criterion $\Sigma S = 0$ and the series criterion "r" = 1.00, the latter is more applicable for the rigid frames considered. The values of the end restraints vary with increase in loading and are unknown. The critical loading for the entire truss frames and groups of members therefrom is found to be greater on the basis of the modified stiffness criterion than from "r." For the entire trusses, this difference in critical load on the basis of E_u for Study No. 1 in each case is about 17 per cent for the airplane truss, 0.4 per cent for the 4 panel truss, and 1.4 per cent for the 6 panel truss. The buckling members of the airplane truss fail at compressive unit stresses considerably below the yield point, while for the civil engineering trusses the critical compressive unit stresses approach the yield point. The total stiffness of each joint decreases in relation to the prevalence of compression members composing the joint and not in proportion to the increase in loading. The first joints to have negative stiffness along the loading range are those which lose their stiffness at the fastest rate. The operations involved in one complete moment distribution for "r" are lengthy for loading approaching the critical value for the entire airplane truss, which consists of a few members, and too long and burdensome for all loadings of the entire 4 and 6 panel trusses, which have more than a few members. Stability analysis of an entire truss frame containing more than a few members is not recommended as a practical computation procedure. Simplified assumptions of analysis of small groups of truss members for approximations to the critical load afford a reduction in the lengthy computations.

Critical Values—Trusses. A summary of the critical values and other related data for the highly stressed compression members of the considered trusses are shown in TABLE 2.

Critical Compressive Unit Stresses. For the airplane truss, the critical compressive unit stresses in both studies are below the proportional limit, with the critical stress factor slightly above 0.5 in the prime member $U_1 U_3$, although about 0.7 in $L_0 U_1$. No definite relation of critical stress to yield point is indicated for trusses of this type, as the slenderness ratios may have considerable variance and failure will occur at stresses considerably below the yield point. The highest critical compressive unit stresses for the civil engineering trusses are from about 1,500 p.s.i. below to about the yield point on the basis of the conservative modulus E_u and practically at the yield point on the basis of E_t. The critical stress factors are close to unity and the compression member having highest unit stress may, therefore, be stressed to practically the yield point. This is of practical importance, since, for the civil engineering trusses, the entailed stability analysis is not required and the yield point may be taken as practically the critical unit compressive stress. The highly stressed compression members of the civil engineering trusses are in the short column range, while for the airplane truss they are in the long column range.

TABLE 2

SUMMARY OF CRITICAL VALUES FOR TRUSSES

							L_0U_1						U_1U_2		$(U_1\ U_3)$			
Truss name	*Study No.*	*Truss type*	*Design formula*	*Yield point p.s.i.*	$\overline{E}$	*Critical load factor*	*Stress p.s.i.*	$\frac{\overline{E}}{1{,}000}$ *p.s.i.*	$\frac{L}{j}$	*Crit. stress factor*	*c.*	*k.*	*Stress p.s.i.*	$\frac{\overline{E}}{1{,}000}$ *p.s.i.*	$\frac{L}{j}$	*Crit. stress factor*	*c.*	*k.*
Airplane	1	Warren	Ultimate	36,000	Eu	1.0310	24,307	24,567	4.392	0.675	1.955	0.715	19,320	27,856	4.462	0.537	2.017	0.704
	2	Warren	Ultimate	36,000	E	1.0452	24,648	24,191	4.457	0.685	1.739	0.758	19,589	27,784	4.499	0.544	2.035	0.701
4 Panel Bldg.	1	Warren	Old A.R.E.A.	36,000	Eu	2.5553	35,035	2,923	3.766	0.973	1.437	0.834	35,681	984	4.108	0.991	1.709	0.765
	2	Warren	Old A.R.E.A.	40,000	Eu	2.8372	38,900	3,209	3.788	0.973	1.454	0.829	39,617	1,138	4.025	0.990	1.641	0.781
	3	Warren	A.I.S.C.	36,000	Eu	2.1509	34,969	3,117	4.103	0.971	1.706	0.766	34,294	5,057	2.051	0.953	0.426	1.532
	4	Warren	A.I.S.C.	40,000	Eu	2.3868	38,805	3,478	4.092	0.970	1.696	0.768	38,055	5,551	2.061	0.951	0.430	1.524
	5	Warren	Old A.R.E.A.	36,000	Et	2.57791	35,343	9,920	2.054	0.982	0.427	1.530	35,998	556	5.489	1.00	3.052	0.572
	6	Warren	A.I.S.C.	36,000	Et	2.21109	35,948	2,796	4.392	0.999	1.955	0.715	35,254	10,569	1.438	0.979	0.210	2.185
	7	Warren	A.I.S.C.	33,000	Et	2.0272	32,960	2,509	4.460	0.998	2.015	0.704	32,321	10,085	1.410	0.979	0.201	2.228
	8	Howe	A.I.S.C.	36,000	Eu	2.1344	34,701	3,897	3.655	0.964	1.354	0.860	35,072	2,814	2.954	0.974	0.884	1.064
	9	Howe	A.I.S.C.	40,000	Eu	2.3680	38,499	4,334	3.651	0.962	1.351	0.861	38,911	3,178	2.928	0.973	0.869	1.073
6 Panel Highway	1	Warren	A.R.E.A. (1943)	36,000	Eu	2.3983	32,209	10,554	3.707	0.895	1.394	0.847	34,536	4,372	3.977	0.959	1.618	0.786
	2	Warren	A.R.E.A. (1943)	33,000	Et	2.2865	30,708	18,545	2.731	0.931	0.756	1.150	32,926	3,332	4.470	0.998	2 024	0 703

Underlined values for member with highest L/j. $(U_1\ U_2)$—airplane truss.

Effective Modulus of Elasticity. For the airplane truss with critical values of E_u that are close to E, the elastic modulus E may be used, with an increase in critical load of 1.5 per cent. This indicates the possibility of simplification in stability calculations when the numerical values of E approximate E. Considering the studies for the civil engineering trusses, on the basis of modern design formulas, the critical E averages about 3.5 million p.s.i. and, for the old design formula, about 1.0 million p.s.i.

Yield Point Ratios. For both the same design formula and modulus concept, the theoretical critical load of the trusses are practically in the same proportion as the yield point ratios.

Critical L/j. The critical L/j values for the airplane truss are close to 4.493, which is for a compression member with one end restrained and the other end pinned or simply supported. For the civil engineering trusses of the Warren type, with E_u, the critical L/j is about 4.0. On the basis of E_t, with the exception of Study No. 5, the critical L/j for the civil engineering trusses is about the same as for the airplane truss with E_u. From Study No. 5, with E_t and the old design formula, the critical L/j is about 5.5. For the civil engineering truss of the Howe type, the critical L/j is about 3.7. Considering the modern design formulas for the civil engineering trusses of the Warren type, the indicated critical L/j is about 4.0 on the basis of E_u and about 4.46 on the modulus concept of E_t.

Prime Compression Members. The prime compression member is not necessarily the member having the highest compressive unit stress. It has the critical value of L/j and the pertinent value of the column end restraint coefficients. At the critical load, there may be compression members that, considered as a unit column, have not reached their full load. It may appear that the member having the highest L/j is the first to fail. When any member as part of a rigid frame becomes unstable, however, the entire structural frame, including all the tension as well as the compression members, lacks stability, as the interdependence of the members which constitute the rigid frame is lost.

Column End Restraint Coefficients. For both studies of the airplane truss, these values may be taken as practically equal to: c = 2.0 and k = 0.7. The first four studies of the 4 panel building truss and the first study of the 6 panel highway truss, of Warren type and with E_u, indicate the k value close to 0.78, with a corresponding c close to 1.65. This k value of 0.78 has been previously suggested for pin-end columns in civil engineering structures, instead of unity, to allow for the friction at the pin joints. Considering the 4 panel building truss of Howe Type, also with E_u, the c and k values are 1.35 and 0.86, respectively. For the studies with E_t, with exception of Study No. 5, the c and k values are about equal to 2.0 and 0.7 respectively, the same as for the airplane truss with E_u. The restraint coefficients indicated in Study No. 5, on the basis of the old design formula with low allowable unit stresses, are about 3.05 and 0.57 for c and k respectively. The restraint coefficients will vary with loading, but of interest in stability studies are the values at the critical load.

Approximate Estimate of Critical Load. The Lundquist method for approximating the critical load has been found applicable to the included trusses. As the critical compressive unit stresses in the civil engineering trusses are close to the yield point, the analytical methods of approximating the estimate of the critical load factor for such trusses may be omitted and instead be readily computed as practically equal to the ratio of the yield point to the highest compressive unit stress at the design load. If the prime compression member of the civil engineering trusses has a small range in load factors between conditions of L/j = π and 2π, the critical load may be estimated within such range. The mentioned restraint coefficients for the prime compression member in the airplane and Warren

Type civil engineering trusses, on the basis of E_u, is a possible practical means of approximating the critical load of these types of trusses.

Groups of Members for Approximations. For the airplane truss, the approximations are limited practically to a group of truss members consisting of a compression member with the terminals twice removed from its ends or to a group with terminals twice removed from the joint of junction of greatest compressive unit stresses at which the unit moment is applied. The latter group has shorter computations for "r." For the 4 panel building truss, the simplest arrangement of a group of members may be used to approximate the critical load within 0.5 per cent of the true value. For the 6 panel highway truss, the simplest group pattern may be used for approximations within 2.0 per cent, and closer approximations result from analysis of a group consisting of a highly stressed compression member with terminals twice removed from its ends or of a group with terminals twice removed from a joint containing the highly stressed compression members and at which joint the moment is applied. The results of the stability analysis of groups of members of the three trusses show the possibilities of this simplified method of approximating the critical load and indicate the general arrangement of member groups that may be used.

Utility of Stability Analysis. Stability analysis has a practical value in the determination of the critical loading, from which the factor of safety of the structure may be obtained, for the airplane truss, which is designed by ultimate load formulas and has compression members in the long column range. For slender columns, instability occurs at unit stresses considerably below the yield point. The restraining effect of the tension members and the reserve strength compression members, which may offer buckling resistance, is unknown. For civil engineering trusses, designed by working load formulas, the critical compressive unit stress may be assumed as practically equal to the yield point, and the stability analysis may be considered futile and be omitted.

Concepts of the Effective Modulus of Elasticity. On the basis of the contained and available data on the several concepts of the effective modulus of elasticity and in the absence of theoretical or test data for this modulus for stresses in typical structural frames, the general deduction is made that the upper limit for this modulus may be the tangent modulus and the lower practical limit may be the modulus evaluated from the ultimate load short column formula. This deduction is premised on the recommendations for the tangent modulus resulting from laboratory tests on ideal columns and on the idea that, to allow for the difference between the ideal and the corresponding practical column, the effective modulus should be of a lower numerical value than the tangent modulus. In the absence of theoretical or test data for the determination of the effective modulus of elasticity for tensile stresses, the usual assumption is made of the same modulus values for equal stresses in either tension or compression. This assumption, on which there is some difference of opinion, is subject to the result of further research to obtain a more rational method of evaluating the effective modulus of elasticity for tensile stresses. The Linear Modulus by Borkman and the Linear Modulus suggested by the author are noted as simplified relations for estimating the effective modulus of elasticity.

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SECTION OF ANTHROPOLOGY[*]

CRUCIAL PROBLEMS IN APPLYING ANTHROPOLOGY

By F. L. W. RICHARDSON, JR.[†]

Useful Concepts for Rough Appraisals—Time and Social Systems

Wrong Predictions. Anyone applying anthropology has assumed a serious burden. He presumes to be proficient in scientifically inducing changes that bring about desired ends. With a little experience in taking responsibility for action, however, he soon finds out whether the plans based upon his theories and hypotheses meet the test of action. If he is honest with himself, he soon realizes that his scientific training is frequently inadequate for the job at hand. The sad truth of the matter is that the theories, insights, and methods of anthropology provide an inadequate basis for scientifically applying anthropology to many human relations problems. Confronted with a wrong prediction, the applied anthropologist derives little satisfaction from the notion that other social and psychological[*] disciplines may be more limited.

Concentrating on Parts of Problems. Errors in predicting commonly arise from excessive preoccupation with a fraction of a total problem. Take, for example, clinical psychologists and psychiatrists who have assumed the responsibility for curing emotionally ill patients. Customarily, they undertake little systematic investigation of the patient's family as a social system and practically no investigation of the business or other systems in which he participates. If the main source of a patient's immediate emotional imbalance happens to lie not in his early family relationships but in repeated frustrations with business colleagues, should not the therapist explore systematically this latter problem rather than attack well-entrenched personal attitudes and value orientations?

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[*] This paper was presented at the meeting of the Section, October 24, 1949. There were no meetings held in December of the Sections of Biology, Psychology, and Anthropology and the Division of Mycology.

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[*] Throughout the paper, psychological refers to psychology, psychiatry and psychoanalysis.

Again, take social engineers, whether social planners, scientists, or reformers, who fix up political and industrial organizations or attempt to improve whole regions and countries. It is not uncommon for these group "doctors" to overlook important parts of a problem. For example, successfully training new foremen to improve their handling of men has backfired when old-time, high-level, and "untrained" foremen later discredited the new practice as "soft." Not only had these latter and higher-placed foremen been overlooked, their habits were of too long standing to be easily changed.

In some undeveloped countries during the war, the introduction of farm machinery to increase food production actually resulted in food decreases as the machinery was diverted by power groups for production of more remunerative cash crops. Not only had the existence of these power groups been overlooked, so too had their long tradition of control for self-interest.

However much preoccupation with fractions of problems may result in inaccurate predictions, how does an applied scientist go about sizing up whole problems?

Viewing Whole Problems. Ideally to encompass a problem fully, it might be desirable to gather all current and historical facts on all individuals affected. But how can this be done practically? How is it possible to gather and analyze myriads of facts involving complex problems and large numbers of people? To show one way of simplifying this task, the writer will present briefly a rough method he has found useful.

The method is based on the concept of the social system.* Roughly speaking, the term "social system" connotes a group of people whose members, during periods of being together, are relatively isolated from nonmembers. As used in these pages, the term "social system" refers to interactions between persons who associate periodically. In small groups, interaction between members is customarily face to face, while in large groups members also are indirectly tied together by interaction through intermediaries.

This concept is by no means static or restricted to one point in time. Instead, it refers to the continuing and changing patterns of people's interactions as they become modified over time. As such, it has been valuable for formulating the following hypothesis: although not precisely understood, it appears that, the more frequently the members within a social system interact, (a) the more uniform their beliefs, attitudes, and behaviour become and (b) the more resistant they become to change. The usefulness of this hypothesis stems from the fact that the members of each social system whether family, church, or nation, ordinarily asssociate sufficiently with one another to acquire similarities in mannerisms, attitudes and other kinds of behaviour. In short, members of a particular social system usually have their own constellation of behavior, attitudes, and symbols. Therefore, in making a rough approximate appraisal of a situation, the members of a particular social system can frequently be considered as a unit on the assumption, for example, that most members will often manifest a similar response to a given stimulus.

Before taking up the discussion of the resettlement project, the writer would like to point out briefly and suggestively the interdependence between social systems and the behaviour and attitudes of members. Consider for a moment the fact that departments of social and psychological disciplines tend to narrow their student's attitudes and views of human relations to conform to that of the system. Then consider that over time these constellations of belief change, a circumstance disturbing to some and not to others. Finally, consider the competition between different social systems for the loyalty, services, and attitudes of given persons who play roles in each. Take, for example, men whose wives frequently urge them to spend more time with their families, whose bosses urge them to greater productive achievements, and whose political bosses occasionally demand long hours from them.

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* HENDERSON, L. J. General Sociology—A Physiologists Interpretation' by V. Pareto. Harvard University Press. Cambridge, Mass. 1935.

So it is with churches, clubs, groups of friends, and any number of other miscellaneous individuals and social groups. In varying degrees, all expect their members to voice "good" attitudes (*i.e.*, the brand of attitudes approved by the system in question).

Amid these rival social planets, little ego tries wending his own individual way. Year in and year out, treading his path between members of social systems, he carries away from each encounter a personal emotional response, and if during such times he is too sensitive to the least force exerted, he oscillates in and out of one social system after another. Some such people take to drink, others to crime, and still others are merely labeled weak or shiftless. Conversely, from the beginning, others resist the attraction of all systems excepting their families and their job or work group, and even here they are consistent in allocating more time to participate with colleagues and less with wife and children. Colleagues often consider such people "good workers" or "serious minded," while their families may refer to them as "poor fathers" or "narrow minded."

Thus, to understand why particular persons, to say nothing of groups, behave and feel as they do, the applied anthropologist should *first* be able to give a rough approximate appraisal of a person's or group's total relationships, *i.e.*, all those who have had an important influence both in the past and present. To do this, some method is needed that simplifies the characterizing of large numbers of people. The method here suggested for this *first stage* of analysis involves: (1) considering people as members of social systems and treating these social systems as units; and (2) considering the historical trends of these social systems.

To gather and analyze such facts, an applied anthropologist must become familiar with all competing social systems into whose orbit the people concerned either have moved or attempted to move or from whose orbit they have been ejected. Rarely, however, does he or any other social or psychological scientist have this knowledge, although each type of social system has, in varying degrees, been described and analyzed by members of one discipline or another. Such specialization may be convenient for academic study by traditional departments, but the applied human scientist who so restricts himself may in many cases find himself elaborating minor parts of total problems.

The Component Parts of Problems. In this first stage, of sizing up human situations roughly, the writer usually lists all types of social systems in order to force him to consider systematically all component parts of a total problem.

For convenience, he divides social systems into three main types—individual systems, institutional systems, and what might be called "areal systems." The relations between one individual and all his associates has been conceived as an individual system. Institutional systems include the following six types: (1) families; (2) cliques; (3) economic systems; (4) political systems; (5) religious systems; and (6) clubs and miscellaneous associations. The areal systems are nothing more nor less than combinations of individual and institutional systems. For convenience, they can be arranged in some such sequence as the following: (1) neighborhoods, tribes, and communities; (2) centers, *i.e.*, villages, towns, and cities; (3) center-suburb systems—including types of settlement ranging from a village with outlying farms to a metropolis with satellite suburbs; (4) counties, states, and regions; (5) nations and international systems.

In the remainder of the paper, we will be concerned with one institutional system, the clique, and one areal system, center-suburbs. These two systems have been chosen for discussion because they are perhaps less discussed than many others and because, in the resettlement project to be mentioned, they played a vital part.

The Case of a Resettlement Project

Failure of one Plan. The example in question is a resettlement project in a mined-out coal area. The writer was intimately connected with this project for about two years, 1939–41, and has visited it several times since. It will not be the purpose of this paper to discuss the project in any detail, as it has, in part, been reported elsewhere.*

The project consisted of about fifty coal miners or so-called "homesteaders," and their families, who beginning in 1937 had moved into this newly opened community called Penncraft. Most of the homesteaders had come from surrounding small mining communities, many of them company-owned. Such a community was locally called a "patch," sometimes a "coal patch" or "company patch," and many of them were run-down communities with closely packed dilapidated shacks; in fact, rural slums. The resettlement project was started by members of a charitable organization who formed a corporation of five or six men who hired a field staff of three. The members of both the corporation and field staff are here referred to as management.

As first conceived, the purpose of the project was to experiment with methods to help coal miners weather depressions more successfully by learning new trades and practising subsistence agriculture. In the course of time, this desire was reformulated to express what became the ultimate goal, namely, to help the homesteaders help themselves to build a relatively self-sufficient community. With the aim of contributing to this main community goal, there were a number of community sub-goals, such as economic and recreational self-sufficiency, homesteader-built housing, and community self-government.

Management succeeded in organizing the homesteaders to build their own houses. Also, they succeeded in launching a sweater factory that employed women and girls and in running a cooperative store. They made little progress, however, in getting the homesteaders to participate in community organizations for such purposes as self-government and recreation.

As documented in part I of the report mentioned previously, participation in community organizations was desultory in 1939 and 1940. This was still true during a week's visit nine years later in September, 1949. Typical comments were, "We don't like goin' to meetings with all that formal stuff," or "to hell with wastin' time down there."

From conversation with the homesteaders, it soon became apparent that they had little experience in community organization, but exactly how they spent their time in the company patches prior to moving remained somewhat a mystery. It was felt that some further clues to their disinterest in community organizations might be forthcoming if a study was made of a typical coal patch. Such a study, it was hoped, would not only show why homesteaders failed to participate in community activities but might possibly suggest what measures could be taken to get them to support actively their own would-be self-governing community organization. At about the same time, a study of the local area, undertaken for a different purpose, accidentally provided the writer with further understanding.

The study of the coal patch was reported in part III of the previously mentioned report and the area study partly in part II, with the remainder reserved for part IV, yet to be published. Points from each, pertinent for this paper, will be briefly summarized here.

One Unappraised Part—the Center-Suburb System. One of the most obvious reasons why homesteaders failed to attend self-governing meetings, community dances, and the like was that their ties lay elsewhere, either with friends at the patch from which they had come or in the beer halls, movie theatres, and other amusement places in the three neighboring towns and cities. There was a town of 2,000 two miles away, and cities of 10,000 and 20,000 four and twelve miles away, respectively.

Even as regards shopping, the homesteaders kept patronizing the company stores at their former patch or at the mine where the husband worked. This, plus the supermarkets in the neighboring cities, provided severe competition for the community cooperative store. As regards schooling, all children left the community, the older ones attending the high schools in the city of 10,000 four miles away. There, in the last few years, night football games with uniformed bands and thousands attending have been an increasingly significant feature. Competition for the band is keen, with even the grade-school girls twirling batons, hoping that someday they will become strutting majorettes. With boys on the squad and team, and both boys and girls in the band, the attention of parents and children alike is centered on the city for the Saturday night games. Recently, to add more excitement, one of the Penncraft boys was a backfield star and, as a result, received a scholarship to go to a university in the big regional center, a city of over a million, less than 50 miles away.

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* RICHARDSON, F. L. W., JR. Community Resettlement in a Depressed Coal Region. J. App. Anth. I Human Problems. 1 (1): 24–53; II Economic Problems. 1 (3): 32–61; III Problem of Community Change. 7 (4): 1–27.

It should be stressed that miners and their families usually drive to town in their own cars; they do not rely primarily on bus service. Anomalous as it may seem, these sometimes impoverished workers, whether employed or unemployed, require cars for commuting to work. The more the coal in the area is mined out, the more the need for cars to drive to work or to new mines, some of which are 20 miles distant. Naturally, the miners often do move closer to their new place of work, but this is not always easy. Housing is scarce, because the coal companies no longer are building company towns. In fact, they are going out of this business and selling the patches they already own. Thus, cars become a necessity for husbands and fathers and, naturally, a convenience for wives and children.

Management obviously recognized the attraction of these town and city centers, but why did they underestimate the strength of their gravitational pull? Management strongly held the belief that Penncraft, with its fifty families, could develop its own gravitational force sufficient to resist the pull of these neighboring urban centers with populations numbering thousands. From the start, management introduced various organizational activities to do just this. In addition to the house building activity, the cooperative store, and the self-governing activities already mentioned, there were three economic enterprises designed to help make the community self-contained—hand-loom weaving, a sweater factory, and a farm. Few were interested in learning weaving, however, while the jobs in the sweater factory were mainly for women and girls, and, although the farm provided milk and produce, it gave steady employment to only one man.

The answer to why management tried to counteract the pull of the cities became apparent only after a study of the area had been completed. In looking into the recent *historical* changes, it was found that the farm villages of a generation ago had largely disappeared. These villages had been real centers for the surrounding farm families who actively patronized the churches, schools, and country store. Now, in contrast, the new farm generation has been increasingly oriented toward the urban centers. Although this shift was obvious, the writer did not grasp the full implications of it until he realized that most members of the project management had grown up in an environment similar to this earlier one, and had a strong emotional attachment to it. They felt that the coal miners, nursed in the "spending-spree" values of the modern urban system, lacked the essential "values" associated with this village system, such as individual initiative and community cooperation. Thus, they sensed that these essential "values" could be inculcated by recreating this past environment. The fact that it might be difficult was no deterrent in light of the great existing need as they saw it. In fact, to attempt doing it in other fashion was too great a fundamental challenge for most members of management to take seriously.

In retrospect, it is embarrassing to make so much of the obvious point that people in suburbs look to their city center and that, unless transportation is lacking or better employment is provided elsewhere, they almost certainly will continue to do so. This point was not obvious to management, however, and an understanding of management's position (vital for every applied scientist) was not meaningful to the writer until (1) the recent historical competition between village and city centers had become clearly established, and (2) the appropriate loyalties, values, and behaviour of both management and homesteaders had become properly associated with one of the systems—those of the homesteaders with the urban system and those of management with the village system. The writer feels that, if he had originally conceived of the center-suburb relationship as a social system, he might have grasped the significance of these facts more quickly.

A Second Unappraised Part—the Clique System. The attraction of the city might in large part explain why homesteaders were drawn away from participating in various community organizations, but it by no means explains the lack of all participation. The town visits of homesteaders were not that frequent. Obviously, there were other factors of a more subtle nature at work, and we set out to explore some of these when we undertook the study of the neighboring coal patch.

For our purposes, the study of the coal patch, with its 146 families, confirmed the suspicions that community organizations were inactive in local patches. In this particular patch, only two community organizations were truly active, both church groups. The surprising discovery to the writer, however, was that functioning kin groups were rare due to population turnover. By and large, the only frequent community contacts between immediate families living in the community was with next-door neighbors, who shouted back and forth from their porches.

Friendship based on propinquity was widespread. In the other words, a system of clique relations existed based primarily on location. However, these locational cliques were by no means mutually exclusive. Individuals sometimes belonged to more than one, with the result that cliques were more or less interlocking, running up one street and down another. Thus, in the competition to attract members, the gravitational pull of cliques was the dominant force, with the apparent exception of the two church groups just mentioned.

Here again, an examination of historical developments disclosed highly significant facts. Actually, the membership of the two successful community organizations was composed of interlocking cliques or groups of neighbors on particular streets or sections thereof, and this had been true from the start. In contrast, some of the unsuccessful community organizations were composed of individuals belonging to widely separated cliques—a situation analogous to Penncraft. In Penncraft, however, cliques barely existed in the first years, because individual homesteader families had been selected with no particular regard for groups of friends and because of management's policy, for example, of discouraging the gathering of "idle" groups.

Based on these current and recent historical observations, the hypothesis is suggested that, at least in communities of this area where functioning kin groups are rare, clique organization is a necessary first stage in the formation of community organizations. If this is true, then one good reason the Penncraft homesteaders failed to support their own self-governing community organizations was that their friendship groups or clique systems were embryonic. In other words, the seeds from which community organizations grow had not yet been sown. To quote from part III, page 22, of the reference mentioned previously:

"Apparently the confidence and informal teamwork gained among a handful of close friends must necessarily precede any effective large-scale organization. Accordingly, cliques form the core of all formal organization and historically speaking they represent the first stage in the evolution of all non-family groups. In short cliques are the initial aggregates around which larger groups can later crystallize.

Unfortunately we know all too little about the intricacies of group behaviour, but the experience we do have demonstrates the fact that people must emotionally accept one another before the habit of cooperation becomes easy—and this emotional acceptance commonly takes place in the informal clique stage.

The remarkable success of the Jewish resettlement communities in Palestine is in no small measure due to a systematic development from closely knit cliques. In Penncraft, had this process been fully realized, we believe the evolution toward effective community organization would have been more direct, more rapid, and more substantial."

Summary and Conclusion

The Concept of the Social System. The job of applied anthropology is to bring about the social changes necessary to achieve a desired end. The crucial problem in this combined art and science is the ability to make reliable predictions—a none too common skill. That predictions fail is perhaps more frequently due to excluding important variables from consideration than from wrongly estimating the variables considered. The reason a particular applied social and psychological scientist excludes important variables from consideration is largely that these variables have not been included in the teachings of his discipline. By so restricting themselves, these "scientists" are acting as *bona fide* members of their own professional social system, with its particular constellation of cosmology, symbols, and beliefs.

In making a rough first appraisal of all the important variables in a total situation, the concept of the social system is an extremely useful abstraction, adding a little more uniformity to our understanding of nature. Many of the otherwise bewildering number of individuals, with their innumerable beliefs, behaviors, and symbols, become partially understood as constellations in one or more social systems.

Unfortunately, anthropology has not exploited this concept as fully as the applied anthropologist could hope. By and large, the "pure" anthropologists have concentrated on describing social systems from the family on one end of the scale to the small local community on the other. To be more useful in applying anthropology, however, the scale must be extended on both ends—on one to include an individual and his personal system and on the other to include regions and nations.

From the discussion of the resettlement project, it was disclosed that a wrong prediction resulted from failure to consider competition from two social systems. On one end of the scale, the clique system had been ignored; on the other, the center-suburb system had been underestimated. The latter, however well known, had to the writer's knowledge never heretofore been explicitly conceived and described as a social system. In short, these two systems, lying on or beyond the periphery of conventional anthropology, proved to be crucial in the solution of this particular community problem.

Time and social systems. Supposing that the writer had been satisfied to explain that the community organizations in the resettlement project had failed because people preferred to remain in their cliques—what then? What could management or homesteaders do with such information? How could they progress from predominantly clique organizations to more inclusive community organizations? Management more or less subconsciously held the belief that cliques are undesirable and not to be encouraged. By studying recent historical changes in a mining town, however, it was discovered that successful community organizations evolved apparently out of cliques. Two or more coalescing cliques supplied the seeds from which these successful community organizations had apparently grown. Thus, the hypothesis was proposed that, in these mining towns, cliques are a necessary first stage in the evolution of community organizations. If this is true, then the steps that lead to eventual community organization should be: (1) encouraging rather than discouraging cliques; and (2) somehow bringing about a coalescence of cliques, so that they can become the nuclei from which community organizations can later evolve.

Similarly, the evolution of the center-suburb system was presented, in which were pointed out the growth from village to city center and the associated change in beliefs, values, and behaviour of members. The significance of these facts to the project was stressed, for management shared the values associated with village centers and the miners shared the values of city centers.

This concludes an outline of some first steps the writer has found useful in applying anthropology to the solution of the human problems with which his clients have been faced. Sometimes, a solution based on this rough first appraisal has appeared sufficient and, in fact, has later been verified by the only real test of a prediction—the test of action. At other times, in more complex situations, it has been necessary to take a second step by proceeding further with more precision to understand the inner functioning of institutional systems and the individual systems of particular persons. This second step has not been examined in this paper. If these rough appraisals or first steps are skillfully executed, however, the required course of action may become so obvious that a second step is superfluous. In short, the point of this paper is that, in sizing up human problems, first steps come first.

REPORT OF THE ANNUAL MEETING

December 7, 1949

The 132nd Annual Meeting of the Academy, for the election of Officers and Fellows, the presentation of reports, and the transaction of other business was held at the Hotel Astor on the evening of Wednesday, December 7, 1949.

The first order of business was devoted to the reports of the Officers and Committees of the Academy.

REPORTS OF OFFICERS AND COMMITTEES

Report of the Recording Secretary. The unprecedented developments of the past year clearly demonstrate that the activities of the Academy, including the Conferences, Sectional Meetings, Membership expansion, and publication program are not only acquiring a considerable momentum, but are the basic factors for achieving our goal with maximum success.

The recognition of the importance of our work by Mr. and Mrs. Norman B. Woolworth has provided the Academy with a home of its own for the first time in more than a century. Through the generous gift of their mansion, an opportunity is presented for the Academy to make a noteworthy stride forward by creating a hospitable gathering place for its Members with adequate accommodations for meetings and administrative activities.

The Recording Secretary is pleased to state that the progress reported last year has continued, although unusual efforts have been required in the conduct of the Building and Sustaining Fund Drive so that the problem of housing the Academy might be solved.

Through the efficient efforts of the Chairmen of the Sections and Conferences, 44 stated Sectional meetings and eight conferences were held during the past 11 months, at which 262 important scientific papers were presented and discussed. Thanks and appreciation are hereby extended to the Chairmen and speakers for the undoubted success of these meetings and the high quality of the papers presented.

The wide range of the subjects dealt with is indicated in the following list of Conferences held:

"The Ground Substance of the Mesenchyme and Hyaluronidase," Chairman, F. Duran-Reynals, Yale University, New Haven, Connecticut.

"The Place of Statistical Methods in Biological and Chemical Experimentation," Chairman, Edwin J. deBeer, The Wellcome Research Laboratories, Tuckahoe, New York.

"Development and Uses of Antimetabolites," Chairman, D. Wayne Woolley, The Rockefeller Institute for Medical Research, New York, New York.

"Coccidiosis," Chairman, Sterling Brackett, Stamford Laboratories, American Cyanamid Company, Stamford, Connecticut.

"Specific Methods of Analysis," Chairman, Samuel E. Q. Ashley, General Electric Company, Pittsfield, Massachusetts.

"International Conference on Vitamin E," Chairman, Karl E. Mason, University of Rochester School of Medicine and Dentistry, Rochester, New York.

"The Chemotherapy of Tuberculosis—The Experimental Approach," Chairman, Geoffrey W. Rake, The Squibb Institute for Medical Research, New Brunswick, New Jersey.

"Mechanism and Evaluation of Antiseptics," Chairman, Herbert L. Davis, Ethicon Suture Laboratories, Johnson & Johnson, New Brunswick, New Jersey.

During the past year 949 new Members were added to our rolls, as follows: 2 Benefactors, 7 Honorary Life Members, 27 Active Life Members, 44 Sustaining Members, 830 Annual Active Members and 39 Student Members. One Active and one Life Member became Patrons and 203 Annual Members were transferred to Life Membership by payment of the Life Membership Fee. Thirteen transfers and 24 reinstatements were effected in the Annual Classes.

The Academy has lost by death 3 Active Life Members, 2 Sustaining Members and 19 Annual Active Members. One hundred nineteen resignations were accepted, 3 names were dropped for nonpayment of dues and 5 for not having qualified for membership.

The record now stands with a net gain of 822 Members. There are, at present, upon the rolls of the Academy 4,852 Members, of whom 609 are Fellows, classified as follows: 3 Benefactors; 3 Patrons; 52 Honorary Life Members; 331 Active Life Members; 167 Annual Sustaining Members; 4,180 Annual Active Members; 116 Annual Student Members.

Report of the Corresponding Secretary. The Corresponding Secretary reported that no changes had occurred in the Honorary Life Membership Class. As of October 31, 1949, 52 Honorary Life Members were enrolled in this Class, which, with the election of seven new members at this Annual Meeting, brings the total to 59 Honorary Life Members.

Report of the Editor. Since the date of the last Annual Meeting, December 15, 1948, The New York Academy of Sciences has issued 9 ANNALS monographs, comprising a total of 1,682 pages. Included in these are 173 papers by 283 authors. The TRANSACTIONS (286 pages) brings the total pagination to 1,968.

The list of publications is as follows:

ANNALS

Volume 50, Article 5, *Thyroid Function As Disclosed by Newer Methods of Study* (12 papers), by J. H. Means *et al.* Pages 279–508. Published January 27, 1949.

Volume 51, Article 3, *Ocean Surface Waves* (15 papers), by B. Haurwitz *et al.* Pages 343–572. Published May 13, 1949.

Volume 51, Article 4, *Molecular Interaction* (17 papers), by R. M. Fuoss *et al.* Pages 573–852. Published May 31, 1949.

Volume 51, Article 5, *Antibiotics Derived from* Bacillus polymyxa (18 papers), by P. H. Long *et al.* Pages 853–1000. Published June 22, 1949.

Volume 50, Article 6, *The Adrenal Cortex* (14 papers), by R. Gaunt *et al.* Pages 509–678 Published June 27, 1949.

Volume 52, Article 1, *The Relation of the Adrenal Cortex to the Structure and Phagocytic Activity of the Macrophagic System* (1 paper), by A. S. Gordon and G. F. Katsh. Pages 1–30 Published July 27, 1949.

Volume 52, Article 2, *Drainage Evolution in the Appalachians of Pennsylvania* (1 paper) by Henry D. Thompson. Pages 31–62. Published August 12, 1949.

Volume 52, Article 3, *Vitamin E* (67 papers), by K. E. Mason *et al* Pages 63–428. Published October 31, 1949.

Volume 52, Article 4, *Coccidiosis* (28 papers), by S. Brackett *et al.* Pages 429–624. Published November 10, 1949

In addition to the articles published during the year, the following are now in press and will be published during the next three months:

Volume 50, Article 7, *Allergy* (10 papers), by A. F. Coca *et al.*

Volume 50, Article 8, *Structure in Relation to Cellular Function* (13 papers), by Robert Chambers *et al.*

Volume 52, Article 5, *The Chemotherapy of Tuberculosis—The Experimental Approach* (20 papers), by Geoffrey Rake *et al.*

Volume 52, Article 6, *The Place of Statistical Methods in Biological and Chemical Experimentation* (16 papers), by E. J. de Beer *et al.*

TRANSACTIONS

Series II, Volume 11, Numbers 1–8, consisting of 286 pages, was completed, printed, and distributed each month from November, 1948, to June, 1949, inclusive.

During the period December 1, 1948–October 31, 1949, the Academy has distributed 65,842 separate publications and 11,109 complete volumes of the various published series, as follows:

Annals	*Separate Numbers*	*Volumes*
Gratis: Members	11,789	
Gratis: Authors, reviews, exchanges and donors	6,831	667
Sales	7,495	65
Authors Reprints	25,600	
Memoirs		
Sales		1
Special Publications, Volumes I–IV		
Gratis: Authors, reviews		16
Sales		391
Scientific Survey of Porto Rico		
Gratis	51	
Sales	175	
Transactions, Series II		
Gratis: Members, authors, exchanges, reviews	6,198	4,377
Sales	252	92
Authors Reprints	7,075	
Bulletins		
Gratis: Members, Affiliated Societies' Members, reviews and gratis	376	5,500
	65,842	11,109

The Academy has received from exchange institutions 6,926 separate publications, which have been duly turned over to the Library of The American Museum of Natural History, in accordance with the purchase agreement between the two institutions. All publications included in the Annals and Transactions, as soon as they are ready for distribution, are sent to the exchange institutions throughout the world.

The total sales value of publications distributed gratis to the Members, authors, donors, reviewers, and exchange institutions is $41,865.49.

Report of the Treasurer. The past year has been highlighted by some of the most substantial contributions in the history of the Academy. By means of these, there has been opened up for the Academy a vista of unprecedented development and growth as an organization of service in its fields of endeavor.

The most outstanding contribution is that of Mr. and Mrs. Norman B. Woolworth. Their gift of 2 East 63rd Street, Manhattan, has given the Academy a permanent home. To implement this gift, and to establish it in operation for the Academy, the Academy's position has been strengthened by gifts of Members through their own Building and Sustaining Fund Drive. Thus, Members and well-wishers in this fiscal year have advanced the Building and Sustaining Fund Drive by contributions of $80,134.22.

Another important gift is that of $10,000 from Mr. A. Cressy Morrison by which he has established The A. Cressy Morrison Prize Fund, the income of which will make possible, in perpetuity, not less than two annual awards of his well-known Prizes in Natural Science.

We gratefully acknowledge with sincere appreciation the donations and grants-in-aid totaling $32,831 toward the costs of the publication program and general expenses during the years 1949 and 1950, as follows: Abbott Laboratories, Inc. $750; American Cyanamid Company $400; Bristol-Myers Company $500; Burroughs-Wellcome Company $1,353; Chas. M. Cox Company $100; Colgate-Palmolive-Peet, Inc. $200; Distillation Products, Inc. $900; Dome Chemicals Company $75; Dow Chemical Corporation $500; E. I. duPont de Nemours & Co., Inc. $300; Freshman Vitamin Company $25; Chas. E. Frosst & Company $250; Hess & Clarke $200; Hexagon Laboratories, Inc. $200; Heyden Chemical Corporation $250; Hoffmann-LaRoche, Inc. $1,400; Hynson, Westcott & Dunning $100; Lambert Pharmacal Company $100; Lederle Laboratories Division, Amer. Cyan. Co. $3,278; Lever Bros., Inc. $250; Eli Lilly & Company $900; McNeil Laboratories $50; The Mennen Company $50; Merck & Company $4,900; Miscellaneous contributions $450; Monsanto Chemical Corporation $400; Nutrilite Products, Inc. $100; Panray Corporation $200; Park & Pollard Company $100; Parke Davis & Company $300; S. B. Penick & Company $100; Chas. Pfizer & Company $3,650; Raymond Laboratories $50; Rexall Drug Company $500; Roche-Organon, Inc. $150; Doctor Salsbury Laboratories $200; R. P. Scheer Co. (Gelatin Products Division) $500; Schenley Industries, Inc. $400; Schering Corporation $250; G. D. Searle & Company $100; R. B. Semler, Inc. $50; Sharp & Dohme, Inc. $1,350; E. R. Squibb & Sons $1,200; Sterling-Winthrop Research Institute $650; Testagar & Co., Inc. $100; The Toni Company $200; The Upjohn Company $1,300; U. S. Vitamin Corporation $200; VCA Laboratories $500; Vitaminerals, Inc. $100; Whitmoyer Laboratories, Inc. $200; Woods Hole Oceanographic Institution $1,500; Wyeth, Inc. $1,000.

The income from annual dues and sales of publications totaled $71,472.00. The Academy's property, including its building, equipment, investments and cash on hand now amounts to $622,780.28 gross.

The expenses show a deficit of $1,803.67. This amount covers an excess of operating expenses of $282.98, plus deductions totaling $1,520.69 for losses through sales of investments. However, this is offset with a sum of $26,501.05 credited as a reserve for 1950, of which $18,746.05 is for grants-in-aid for publications and $7,755 reserved for operating expenses, representing one-sixth of the income from 1949 Annual dues.

The fiscal year was changed to begin as of November 1st each year and to end the following October 31st. During the past year, the books of accounting have been established on an accrual basis instead of a cash basis, as heretofore. This work has been especially facilitated by the addition to the Administrative Staff of Mr. Eugene Doerr, as Accountant.

This comprehensive review of our financial position, favorable as it is, should not be taken as indicating that the Academy is now affluent and, therefore, in no need of additional contributions toward its goal. Nothing could be further from the truth. The fundamental facts are that the acquisition of the new building has entailed greater responsibilities and immediate problems to be faced, involving alterations for its use as a public building as required by the City of New York, as well as expenses of moving, equipment, and maintenance for our activities. The Academy also has undertaken the liability of a $200,000 first mortgage on the property with payments to be made on annual interest and amortization. Nevertheless, with the continued and generous support of the Members and friends of the Academy, combined with the aid of the loyal and hardworking Members of the Council and the Academy's staff, our progress toward success should be assured.

A summary of the auditor's report is presented here which outlines the Academy's financial affairs during the fiscal period ending October 31, 1949. In this instance, it represents a period of eleven months, due to the advancement of the opening date of the fiscal year.

By order of the Council, the books of account have been checked and certified by a professional firm of auditors. The property of the Academy has been verified by the auditors and the Finance Committee. This report has been approved by the Finance Committee as of December 1, 1949, as provided in the Bylaws.

Report of the Fund Drive Advisory Committee. The Advisory Committee on the Building and Sustaining Fund campaign considers this a fitting time to present a report to the Members on the progress of the Fund Drive.

After a slow start, the Drive acquired momentum during this year so that now we can state the receipt of contributions and pledges, as of October 31, 1949, amounted to $80,134.22. This includes $68,134.22 resulting from the contributions of 586 Members, including Life Membership and Patron fees; $11,000 from the American Cyanamid Company; $9,544.31, transfer of the William Otis Sweet Fund as Mr. Sweet's contribution to the Building Fund; and, $1,000 from the Granthale Foundation.

THE NEW YORK ACADEMY OF SCIENCES

Balance Sheet

October 31, 1949

	Total	*General Fund*	*Grants-in-Aid Fund*	*Endowment Funds*	*Morrison Prize Fund*	*Bldg. & Sustaining Fund*
ASSETS						
Cash in Chase National Bank	$24,030.95	$441.41	$13,296.90	-	$262.50	$10,030.14
Accounts receivable—publications	3,009.33	3,009.33	-	-	-	-
Membership dues receivable	2,003.50	2,003.50	-	-	-	-
Investments	95,496.69	416.25	-	$45,535.95	7,284.38	42,260.11
Real estate—2 East 63rd Street	425,035.00	-	-	-	-	425,035.00
Office furn. & equipment at cost	6,513.49	5,804.57	-	-	-	708.92
Building fund pledges receivable	20,342.67	-	-	-	-	20,342.67
Prepaid expenses	356.85	114.01	-	-	-	242.84
Interfund loans receivable	45,991.80	268.11	5,586.42	40,000.00	-	137.27
Total Assets	$622,780.28	$12,057.18	$18,883.32	$85,535.95	$7,546.88	$498,756.95
LIABILITIES						
Accounts payable	$7,710.47	$7,710.47	-	-		-
Deferred income—dues	7,755.00	7,755.00	-	-		-
Mortgage payable	200,000.00	-	-	-		$200,000.00
Interfund loans payable	45,991.80	5,586.42	$137.27	$268.11		40,000.00
Total Liabilities	$261,457.27	$21,051.89	$137.27	$268.11		$240,000.00
RESERVES						
For delinquent dues	$1,346.00	1,346.00	-			
For unexpended grants	18,746.05	-	$18,746.05			
Total Reserves	$20,092.05	$1,346.00	$18,746.05			
CAPITAL						
Capital Funds	$341,230.96	($10,340.71)	-	$85,267.84	$7,546.88	$258,756.95
Total	$622,780.28	$12,057.18	$18,883.32	$85,535.95	$7,546.88	$498,756.95

THE NEW YORK ACADEMY OF SCIENCES

Statement of Income and Expense

From December 1, 1948 to October 31, 1949

	Total	General Income Account	Grants-in-Aid	Endowment Funds	Morrison Prize Fund	Bldg. & Sustaining Fund
Income						
Annual Membership dues	$39,816.75	$39,816.75	-	-	-	-
Sale of Academy publications	23,900.25	23,900.25	-	-	-	-
Contributions & grants-in-aid	333,344.45	416.25	$32,831.00	$393.82	$7,284.38	$292,419.00
Dinner subscriptions	1,307.75	1,307.75	-	-	-	-
Library exchange, Amer. Mus. Nat. Hist.	100.00	100.00	-	-	-	-
Investment income	4,750.98	2,962.36	-	215.49	262.50	1,310.63
Interest on bank funds	26.12	-	-	-	-	26.12
Total Income	$403,246.30	$68,503.36	$32,831.00	$609.31	$7,546.88	$293,755.75
Expenses						
Publication costs	$48,401.17	$37,520.17	$10,881.00	-		-
General administrative expense	26,126.16	26,126.16	-	-		-
Meetings & Conferences	7,743.96	4,540.01	3,203.95	-		-
Prizes	800.00	600.00	-	$200.00		-
Building Fund Drive expenses	6,395.03	-	-	-		$6,395.03
Total Expenses	$89,466.32	$68,786.34	$14,084.95	$200.00	-	$6,395.03
Unexpended balance	$313,779.98	($282.98)	$18,746.05	$409.31	$7,546.88	$287,360.72
Less: Loss on stock sales	1,520.69	1,520.69	-	-	-	-
Total Income over Expenses	$312,259.29	($1,803.67)	$18,746.05	$409.31	$7,546.88	$287,360.72
Allocated to Capital Funds	$293,513.24	($1,803.67)	-	$409.31	$7,546.88	$287,360.72
Balance to Reserve	18,746.05		$18,746.05			
Total (as above)	$312,259.29					

The Academy's position was most substantially improved by the gift of Mr. and Mrs. Norman B. Woolworth of their home. Through their generosity, the Academy has now come into possession of a building, adding a net contribution from Mr. and Mrs. Woolworth of $225,000.

We are thankful to these benefactors, the Members, and the friends of the Academy for this progress so necessary for the development of the Academy's potentialities.

While we take pride in our growing possessions, the completion of our project is a matter of necessity We need to build up our Sustaining Fund that we may broaden the scope of our activities and carry on our publication program successfully. Although our building is in excellent condition, nevertheless certain alterations must be made as prescribed by the City of New York. Adequate furnishings and equipment are sorely needed and the responsibility of the mortgage must be carried until it is finally amortized.

This is a program which becomes the second phase of the Drive and is in itself an important and arduous task.

Its completion will require additional supporters and further contributions, as well as volunteers to aid us in extending this part of the Drive. We are just as dependent upon the gifts and generosity of our many Members and friends as heretofore.

If we continue in a spirit of determination, with belief in our future and our high ideals, this Committee will look forward to reporting upon a still greater measure of success at our next Annual Meeting.

AWARDS OF PRIZES

Two prizes of $200 each in The A. Cressy Morrison Prize Competition in Natural Science were offered for the year 1949.

Twelve papers were presented, all of high quality. After careful consideration and consultation with experts in the fields represented, the Committee makes the following awards:

(1) A prize of $200 to the paper entitled "Stability Studies of Structural Frames," by Abraham Slavin, Consulting Engineer, Department of Civil Engineering, Polytechnic Institute, Brooklyn, New York.

(2) A prize of $200 to the paper entitled "Embryology of the Viviparous Insects," by Harold R. Hagan, 63 Adrian Avenue, New York, New York.

(3) Honorable mention to the paper entitled "Comparative Biology of Salticid Spiders at Rancho Grande, Venezuela. Part IV. An Analysis of Display," by Jocelyn Crane, Research Zoologist, Department of Tropical Research, New York Zoological Society, New York, New York.

The Committee is pleased to announce that Mr. Morrison, in establishing The A. Cressy Morrison Prize Fund, has made it possible to devote its income to the annual renewal of his offer of two prizes of $200 each, for the two most outstanding papers in a field of natural science covered by the Academy and its Affiliated Societies. Therefore, on behalf of Mr. Morrison, these two prizes will be offered for competition during the year 1950. Announcement of these awards will be made at the Annual Meeting of the Academy to be held in December, 1950.

ELECTION OF FELLOWS AND HONORARY LIFE MEMBERS

The following Members were elected to Fellowship:

Anthony A. Albanese, Ph.D.
Henry Aldrich, Ph.D.
William Antopol, M.D.
Joseph W. Beard, M.D.
James H. Birnie, Ph D.
Morton S. Biskind, M.D.
Ilsley Boone, B.S.
John R. Bowman, Ph.D
Sterling Brackett, Ph.D.
Joseph H. Bunim, M.D.
Abraham Cantarow, M.D.
Harold G. Cassidy, Ph.D.
I. L. Chaikoff, M.D.
Clarence C. Clark, Ph.D.
Beverly L. Clarke, Ph.D.
Amando Clemente, Ph.D.
Amador Cobas, Ph.D.
Seymour Stanley Cohen, Ph.D
W. M. Copenhaver, Ph.D.
Ivor Cornman, Ph.D.
Theodore J. Curphey, M.D.
Herbert L. Davis, Ph D.
Irene Corey Diller, Ph.D.
Richard Donovick, Ph.D.
Thomas F. Dougherty, Ph.D
Victor A. Drill, M D.
Francesco Duran-Reynals, M.D
William R. Duryee, Ph.D.
Philip Eisman, Ph.D.
Herbert Elftman, Ph.D.
Conrad A. Elvehjem, Ph.D.
Gladys A. Emerson, Ph D.
Earl W. Flosdorf, Ph.D
Jay T. Fox, Ph.D.
John P. Fox, Ph D.
Mario Gaudino, M.D.
Arnold Gesell, Ph D.
Morris Greenberg, M.D.
I. C. Gunsalus, Ph.D.
Florence A. Heisler, Ph.D.
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Hadley Kirkman, Ph.D.
Benjamin Kramer, M.D.
Charles A. Kraus, D.Sc.
Martin Kuna, M.S.
Max A. Lauffer, Jr., Ph D.
Robert F. Light, M.S.
Milton Levy, Ph.D.
S. E. Luria, M.D.
Ross A. McFarland, Ph.D
Karl E. Mason, Ph.D.
James E. Miller, M.D.

George S. Mirick, M.D.
Philip McC. Morse, D.Sc.
John S. Nicholas, Ph.D.
Clara Nigg, Ph.D.
Alex B. Novikoff, Ph.D.
I. A. Parfentjev, Ph D.
Arthur K. Parpart, Ph.D.
Karl E. Paschkis, M D
Ernest Peter Pick, M.D.
Abou D. Pollack, M.D.
Eric Ponder, M.D.
Keith R. Porter, Ph.D.
Robert W. Ramsey, Ph.D
Cornelius P. Rhoads, M.D.
J. M. Ruegsegger, M.D.
Bela Schick, M.D.
Leon H. Schneyer, Ph D.
Malvina Schweizer, Ph.D.
Stuart L. Seaton, B.S.
Beatrice Carrier Seegal, M.D.
A. M. Shanes, Ph.D.
Charles A. Slanetz, Ph.D.
Philip A. Stansly, Ph.D.
Abraham Stone, M.D.
Marion B. Sulzberger, M.D.
Wilbur W. Swingle, Ph.D.
John T. Tate, D.Sc.
Hugh S. Taylor, D.Sc.
Kenneth Wade Thompson, M.D
William S. Tillett, D.Sc.
John W. Tukey, Ph.D.
Wayne W. Umbreit, Ph.D.
Victor Vacquier, M A.
Emery Imre Valko, Ph D
Erwin P. Vollmer, Ph.D.
Matthew Walzer, M.D.
Alan T. Waterman, Ph.D.
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William F. Wenner, Ph D
Abraham White, Ph D
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Emil Witschi, Ph.D
Stewart G. Wolf, Jr., M D.
Robert B. Woodward, Ph.D
John Lamar Worzel, Ph.D.
Irving Wright, M.D.

Honorary Life Membership was conferred upon the following eminent scientists:

Sir Thomas Dalling, Animal Diseases, Chief Investigator, Animal Disease Research Association, The Animal Health Trust, London, England.
Sir David Kennedy Henderson, Psychology, Professor of Psychiatry, University of Edinburgh, Edinburgh, Scotland.
Albert Jan Kluyver, Microbiology, Professor of Microbiology, Technical University, Delft, The Netherlands.
Kaj Ulrick Linderstrom-Lang, Chemistry, Director, Carlsberg Laboratory, Copenhagen, Denmark.
Jean Piaget, Psychology, Professor of General Psychology, Lausanne University, Lausanne, Switzerland.
Carl-Gustaf Arvid Rossby, Meteorology, Professor of Meteorology, University of Chicago; Director, Hógskolas Institut fur Meteorologi, Stockholm, Sweden.
Felix Andries Vening-Meinesz, Hydrology, Professor of Geodosy and Geophysics, University of Utrecht, The Netherlands.

ELECTION OF OFFICERS

The following officers were elected for the year 1950:

President

M. L. Crossley

Vice-Presidents

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Ross A. Baker

Chairmen of Sections

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John A. Aeschlimann
Erich M. Schlaikjer

Finance Committee

Addison Webb, *Chairman*
Gordon Y. Billard
Harden F. Taylor

PROGRAM OF THE EVENING

After the Business Meeting, following program was given:

Colloids of the Atmosphere and Laboratory Monodisperse Colloids. By Victor K. LaMer, Department of Chemistry, Columbia University.

The Production of Snow and Rain in Cloudy Skies. By Vincent J. Schaefer, General Electric Company, Schenectady, New York.*

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* Abstracts of these papers have not been received.

STABILITY STUDIES OF STRUCTURAL FRAMES

By ABRAHAM SLAVIN

Awarded an A. Cressy Morrison Prize, 1949

[An abrigment of this paper was presented at a meeting of the Section of Physics and Chemistry of The Academy. As the abridgment appears in this issue (pp. 82–98), the abstract is omitted here.]

EMBRYOLOGY OF THE VIVIPAROUS INSECTS

By HAROLD R. HAGAN*

Awarded an A Cressy Morrison Prize, 1949

A monograph giving for the first time a summary of the historical and embryogenic details of all viviparous insects published to date. Many others are treated historically. New contributions by the author are included: one new viviparous embryology (Diploptera) appears for the first time, another (Arixenia) is given less fully and a third (Glossina) has had further data added to its known embryonic history. Introductory chapters on terminology and general hexapod development together with the method of presentation should enable the beginner to follow the text while the detailed, thorough treatment will satisfy the professional entomoligist. So many orders possess viviparous species that the work will prove a valuable guide in the study of the development of most insects. Theoretical as well as factual considerations affecting the viviparous condition receive proper attention. There are 232 figures in the 160 illustrations and a comprehensive bibliography covers all pertinent literature on the subject.

Of the fifteen chapters in the text, three are devoted to the clarification of embryological terms and a general consideration of insect embryology to prepare the beginning student that he may grasp the morphological details to follow. These chapters also tend to render the volume suitable for a text as well as a reference work for the professional entomologist. Chapter four reviews the types of viviparity among insects and distributes the species among the four functional types discovered. These types and the species in them are assorted purely on physical and physiological interrelationships exhibited by the embryos, their mothers, or both. Chapters five to fourteen contain all the known viviparous embryogenies, and historical summaries of many whose viviparity has been erroneously cited or whose embryogenies remain to be studied. The last chapter brings together, in summary form, the several essential facts of viviparity and their possible bearing on embryonic development, the immature offspring, the mother and the species as a whole.

The viviparous condition seems to modify trophic, instinctive, morphological and psychic responses to insects, or to be possible as a result of such modifications. The author supports the second alternative. Viviparity is a relatively new condition in insects and seems to be replacing oviparity in several orders. It apparently reduces environmental resistance in species possessing it. To support the descriptive matter there are 232 inked drawings in 160 text figures.

COMPARATIVE BIOLOGY OF SALTICID SPIDERS AT RANCHO GRANDE, VENEZUELA. PART IV. AN ANALYSIS OF DISPLAY

By JOCELYN CRANE*

Awarded Honorable Mention in the A. Cressy Morrison Prize Competition, 1949

A comparative analysis of courtship and threat displays in about fifteen species of neotropical jumping spiders resulted in the following major conclusions: first, a number of previously unrecognized elements, both internal and external, are important in the innate releasing mechanism; second, not only the form, but also the relative importance of the factors of the external stimulus configuration varies within the family; third, these variations appear often to be of phylogenetic significance.

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* Department of Biology., College of the City of New York, New York, N. Y.

* Department of Tropical Research, New York Zoological Society, New York, New York.

Throughout the family, vision is the dominant sense, as previous investigators have agreed. Nevertheless, in those genera which are held to be the more primitive, it appears that far greater dependence is placed on chemical senses than in more advanced forms. Contact chemical stimuli play minor parts in courtship throughout the family. Evidence is presented that, contrary to existing views, these spiders are sensitive also to airborne chemical stimuli which play an important secondary role with respect to vision. It appears that various salticid subfamilies tended to evolve through the same general pattern, from a high dependence on all chemical stimuli through a stage in which airborne chemical stimuli are of relatively greater importance, to complete visual domination, in which the chemical senses are relegated to an extremely subservient role.

Among the visual factors, motion, general form, and size take precedence over all secondary sexual "ornamentation," although the latter is, to human eyes, often the most conspicuous characteristic of the spider. In several exceptional instances, however, striking patterns were shown experimentally to have high releasing or directive value for certain stages of threat or courtship. Although the majority of conspicuous epigamic "decorations" did not appear to have a definite function in display, they are not, of course, necessarily non-adaptive characters in this field. Some doubtless merely present a typical example of the unequal value of elements in the contemporary configurational stimulus situation and of the absence of a lock-key releasing formula. The effective configuration is made up of many small bits, including both vestigial and recent elements. Together they have cumulative releasing, directive or merely excitatory value; but a number may be missing from an individual situation without noticeable effect, even when other elements are not reinforced.

It appears that in the most primitive forms, inter-male display does not exist; in the intermediate groups it is scarcely differentiated from courtship, arises from mistaken identity and frequently ends in true fighting; while in the most advanced forms inter-male display has been highly ritualized and is completely distinct from courtship. In these cases it perhaps has a stimulatory function similar to that which apparently occurs in certain birds. Releasing and directive factors in threat display appears to be altogether non-chemical.

Concepts of dominance, sociality, territoriality and displacement behavior seem to apply on a primitive level to certain salticids. No evidence was found, however, of display learning, imprinting or copying. Display motions probably evolved from a number of actions occurring in the ordinary life of the spider. It is suggested that among salticids display actions in general precede the associated morphological specializations, and that they probably play a secondary role in speciation.

NEW MEMBERS

Elected December 15, 1949

LIFE MEMBERSHIP

Fitzpatrick, Florence K., Ph.D., Rickettsiae, Viruses, Tuberculosis. Research Bacteriologist, Sharp and Dohme, Inc, Glenolden, Pa.

Grossman, Cecilia M., M.A, Biology, Education. Teacher of Biology, Far Rockaway High School, Far Rockaway, L. I., N. Y.

SUSTAINING MEMBERSHIP

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Feldman, Samuel, M.D., Peripheral Vascular Disease. Associate, Peripheral Vascular Diseases; Chief, Peripheral Vascular Clinic, Beth El Hospital, Brooklyn, N. Y.

Jacoby, Kurt, Biochemistry, Chemistry, Biology. Secretary-Treasurer, Academic Press Inc., New York, N. Y.

Scherlis, Leonard, M.D., Cardiology, Internal Medicine. Resident in Cardiology, Mt. Sinai Hospital, New York, N. Y.

ACTIVE MEMBERSHIP

Aitken, Gerard J., Jr., M.D., Medicine. Fellow in Medicine, Queens General Hospital, Jamaica, L. I., N. Y.

Arena, Joseph A., Jr., M.D., Medicine. Fellow in Surgery, Alexander Blain Hospital, Detroit, Mich.

Bauer, Louis Hopewell, M.D., Medicine. Practicing Physician, Hempstead, L. I., N. Y.

Borun, E. Raymond, M.D., Cardiology. Resident in Cardiology, Mt. Sinai Hospital, New York, N. Y.

Burnham, Philip C., M.A., Pharmaceutical Chemicals. Secretary and Director, R. W. Greaff and Company, Inc., New York, N. Y.

Cordero, Narciso, M.D., Physiology and Biochemistry. Associate Professor of Physiology and Biochemistry, University of the Philippines, Manila, P. I.

Dayrit, Conrado, M.D., Pharmacology, Cardiology. Instructor in Pharmacology, College of Medicine, University of the Philippines, Manila, P. I.

Dowdy, Andrew H., M.D., Medicine, Radiology, Cancer, Nuclear Physics. Professor of Radiology and Chairman of the Department, University of California, Los Angeles, Calif.

Durivage, Jacques, M.D., Rheumatic Diseases. Research Fellow, Cornell University Medical College, New York Hospital, New York, N. Y.

Ferlauto, Robert J., B.A., Bacteriology. Head, Bacteriology Section, Smith, Kline & French Laboratories, Philadelphia, Pa.

Garbutt, Raymond J., D.V.M. Chief Veterinarian, Hospital of the A.S.P.C.A.; Private Practitioner, New York, N. Y.

Gendron, Pierre R., Ph.D., Colloid Chemistry. Research Assistant, Columbia University, New York, N. Y.

Greathouse, Glenn A., Ph.D., Biochemistry and Biophysics. Director, National Research Council, Prevention of Deterioration Center, Washington, D. C.

Hackett, Walter Thomas, M.D., Pharmacology. Student of Medicine, University of Chicago, Chicago, Ill.

Halley, Leroy F., Ph.D., Chemistry and Bacteriology. Johnson and Johnson, New Brunswick, N. J.

Hantman, Harris W., D.V.M., Public Health. Student of Medicine, University of Zurich, Zurich, Switzerland.

Harris, Jack, B.S., Electrical and Mechanical Instruments. Chief Engineer in Charge of Research and Development, Dinson Clock Corporation, Jamaica, L. I., N. Y.

Henkelckian, H., Ph.D., Sanitation. Professor and Research Specialist in Sanitation, Rutgers University, New Brunswick, N. J.

Komrad, Eugene L., A.M., Physiology. Graduate Student, Research Fellow in Physiology, Boston University School of Medicine, Boston, Mass.

Krakauer, Henry R., M.D., Biology and Chemistry. Assistant Resident, Neoplastic Disease Service, Montefiore Hospital, New York, N. Y.

Lawner, Rhoda, M.A., Psychology. Lecturer, Department of Psychology and Philosophy, Hunter College, New York, N. Y.

Meyer, Ruth Judith, B.A., Biology. New York, N. Y.

Misrahy, George A., M.D., Cardiovascular Diseases. Assistant Physician, Medical Center, New York, N. Y.

Morrison, David, Psychology. Research Engineer, Flight Safety Foundation, Inc., New York, N. Y.

Nordmann, Jo., M.D., Biochemistry. Chief of Laboratory, Clinique Chirurgicale, Paris, France.

Ower, John James, M.D., Pathology. Professor of Pathology, University of Alberta, Edmonton, Alta., Canada.

Pfeiffer, Ehrenfried E., M.D., Microbiology of Soil, Blood Testing. Director, Biochemical Research Laboratory, Spring Valley, N. Y.

Plotnick, Morris, B.S., Biology. University of Cincinnati, Cincinnati, Ohio.

Randolph, Malcolm L., Ph.D., Ultracentrifuge, Electrophoresis. Assistant Professor of Physics, Tulane University, New Orleans, La.

Rommer, Richard J., M.S., Geology and Meteorology. Instructor of Geology and Meteorology, College of the City of New York, New York, N. Y.

Salomon, Kurt, Ph.D., Biochemistry, Pharmacology. Assistant Professor of Radiation, Biology and Biochemistry, University of Rochester, Rochester, N. Y.

Sarett, Herbert Paul, Ph.D., Biochemistry. Assistant Professor of Biochemistry, Department of Biochemistry and Medicine, Tulane University School of Medicine, New Orleans, La.

Schubert, Jack, Ph.D., Physical and Biological Chemistry. Senior Chemist, Argonne Medical Laboratory, Chicago, Ill.

Sheinkopf, Jack A., M.D., Cardiovascular Diseases Instructor, University of Southern California School of Medicine, Los Angeles, Calif.

Stein, Edward, B.A., Physiology and Biochemistry Teaching Fellow in Physiology, New York University College of Dentistry, New York, N. Y.

Steinberg, Frederick W., M.D., Physics and Chemistry Chief of Medicine, Nix Tumor Clinic, San Antonio, Texas.

Taylor, Robert R., M.A., Nutrition, Physiology Research Research Biochemist, Lederle Laboratories Division, American Cyanamid Company Pearl River, N. Y.

Weitzner, Arthur, B.A., Medicine Candidate in Medicine, Faculty of Medicine, University of Geneva, Geneva, Switzerland

Yager, Alfred, M.D., Medicine Fellow in Pathology, Beth Israel Hospital, New York, N. Y.

ADDITIONS TO LIFE MEMBERSHIP CLASS BY TRANSFER

Daniel V. Wadsworth Angela DuBois Gregory

SER. II, VOL. 12 February, 1950 No. 4

TRANSACTIONS
OF
THE NEW YORK ACADEMY OF SCIENCES

Editor
ROY WALDO MINER

Associate Editor
WEBSTER BRIGGS

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Executive Director: EUNICE THOMAS MINER

SECTION OF BIOLOGY[*]

STUDIES ON PTEROYLGLUTAMIC ACID INHIBITORS[†]

By J. J. OLESON[‡]

The use of biologically important substances, and derivatives prepared therefrom, often extends beyond their function as originally discovered when a sufficient quantity of a variety of related compounds becomes available for diverse research and clinical investigations. One example of this is pteroylglutamic acid (PGA) and its derivatives. Originally a growth factor for bacteria and chickens, these compounds are now being used for the treatment of a variety of macrocytic anemias and neoplastic diseases and in fundamental biological work on many species. We shall present here a few experiments on a limited number of derivatives and some nutrition experiments involving some of them.

TABLE 1

THE ACTIVITY OF VARIOUS PGA DERIVATIVES FOR CHICK GROWTH

Active compounds	*Inactive compounds*
pteroylglutamic acid	"Hydrolysis" products of PGA
pteroyldiglutamic acid	pteroyl d(−) glutamic acid
pteroyltriglutamic acid	pteroylserylglutamic acid
pteroylhexaglutamic acid	pteroylaspartic acid
pteroylglutamyl glycine	pteroyl α aminopimelic acid
pteroylglutamyl-β alanine	pteroyl (dl) threonine
formyl pteroylglutamic acid	2,4 dihydroxy pteroylglutamic acid
N-10 nitroso pteroylglutamic acid	7 hydroxy pteroylglutamic acid
	7 methylpteroylglutamic acid
	9 methylpteroylglutamic acid
	9-10 dimethylpteroylglutamic
	thymine
	xanthopterin
	pteroic acid

TABLE 2

COMPARISON OF PGA INHIBITORS

Strong (*10-100* γ*/day*)	*Moderate* (*100* γ*-1 mg./day*)
4 aminopteroylglutamic acid (aminopterin)	4 aminopteroylaspartic acid
4 aminopteroyltriglutamic acid	4 aminopteroyl α alanine
4 aminopteroylglutamyl glycine	4 aminopteroyl threonine
4 aminopteroylglutamyl β alanine	9-10 dimethylpteroylglutamic acid
4 amino 9 methylpteroylglutamic acid	6 methyl 7 pteroylglutamic acid
4 amino 9-10 dimethylpteroylglutamic acid	7 methylpteroylglutamic acid
	4 aminopteroylserylglutamic acid

Weak or Inactive (*>1 mg./day*)
pteroylaspartic acid
4 dimethylaminopteroyl glutamic acid
pteroyl d(−) glutamic acid
pteroylserylglutamic acid
dichloropteroylglutamic acid
2,4 dihydroxy pteroylglutamic acid
N-10 nitroso aminopterin

The relation of structure to growth promoting activity in chicks fed a synthetic PGA-deficient diet is shown in TABLE 1. As is well known, the intact pteroylglutamyl moiety is essential for activity.

---

[*] The Section of Geology and Mineralogy held a meeting on Jan. 2, 1950, at which Robert L. Nichols, Tufts College, Medford, Mass., presented a paper entitled "Beyond the Arctic and Antarctic Circles," illustrated with Kodachrome slides. No abstract of this paper has been received.

[†] This paper, illustrated with lantern slides, was presented at the meeting of the Section on January 9, 1950.

[‡] Lederle Laboratories Division, American Cyanamid Company, Pearl River, N. Y.

TABLE 3

EFFECT OF B_{12} ON REVERSAL OF ANTAGONIST

Group	*Supplement to basal diet*	*Avt. wt. & no. alive (—) 0 weeks*			
		Initial	*1*	*2*	*3*
1	none	41 (8)	62	74	81 (6)
2	5 γ PGA	41 (8)	79	134	212
3	25 γ PGA	41 (8)	81	127	196
4	0.2 γ B_{12}	41 (8)	76	97 (7)	114 (4)
5	0.2 γ B_{12} + 5 γ PGA	40 (8)	79	139	218
6	0.2 γ B_{12} + 25 γ PGA	40 (8)	77	131	209
7	none	41 (8)	70	86 (4)	90 (2)
8	5 γ PGA	40 (8)	76	133	191
9	25 γ PGA	41 (8)	77 (6)	137	198
10	0.2 γ B_{12}	41 (8)	77	107	104
11	500 γ 4 aminopteroylaspartic acid	44 (8)	53	72 (1)	— (0)
12	as 11 + 5 γ PGA	44 (8)	62	91 (7)	91 (7)
13	as 11 + 25 γ PGA	44 (8)	72	126	178
14	as 11 + 0.2 γ B_{12}	44 (8)	59 (7)	61 (6)	52 (1)
15	as 11 + 0.2 γ B_{12} + 5 γ PGA	43 (8)	69	106 (6)	163 (6)
16	as 11 + 0.2 γ B_{12} + 25 γ PGA	43 (8)	79	141	212 (8)

Alteration of the amino acid portion or the 2 amino or 4 hydroxy groups inactivates the compound, as does the introduction of methyl groups at various positions. Addition of other amino acids, in peptide linkage to the molecule, gives active compounds, but interposing a different amino acid between the pteroic and glutamic acids does result in inactivity. It is of interest that the N-10 formyl and N-10 nitroso substituents yield fully active compounds.

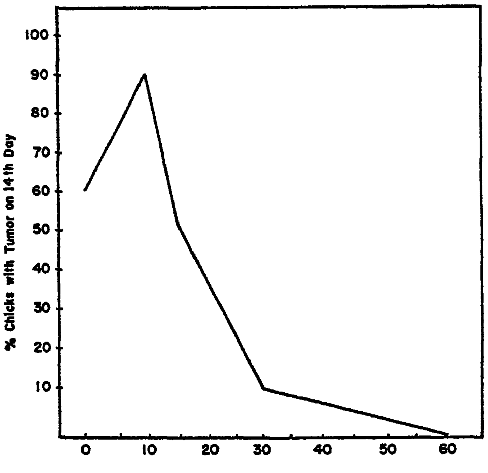

FIGURE 1 Effect of protein on growth of Rous

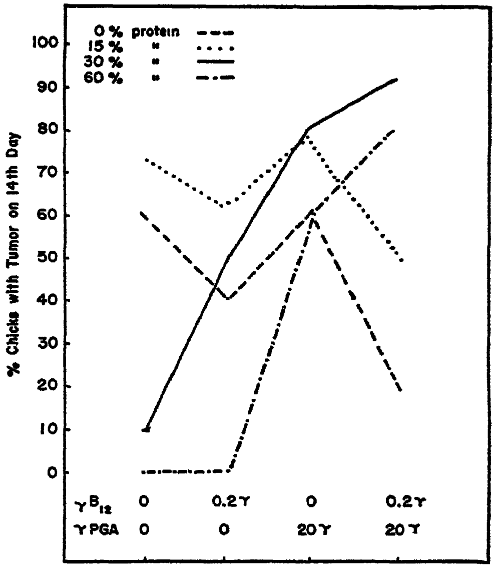

FIGURE 2 Effect of PGA and B_{12} on Rous in chicks fed various protein levels

Certain changes in the structure of PGA produce extremely potent antagonists which are reversed by proper levels of the vitamin, albeit with difficulty in some cases. TABLE 2 lists some compounds tested as inhibitors using the growth of chicks and rats. They are arbitrarily listed in 3 groups according to the amount of compound necessary to produce inhibition of growth.

The most potent inhibitors are those containing the 4 aminopteroylglutamyl moiety. Conjugation with other amino acids or addition of methyl groups does not markedly weaken the 4 amino type of compounds. However, the substitution of other amino acids for the glutamic acid in the 4 amino series somewhat lessens their inhibitory potency. The methylated PGA derivatives are moderately active for chicks but very weak for rats, which are less susceptible to these compounds. It is interesting that the 4 dimethylamino and the N-10 nitroso derivatives of aminopterin are inactive as antagonists.

TABLE 4

THE INHIBITORY EFFECT OF 4-AMINO COMPOUNDS ON THE ACTIVITY OF PTEROYLGLUTAMIC ACID IN GROWTH OF ROUS SARCOMA

Compound	*Am't inj. daily/am't of PGA*	*Total no. of chicks*	*% chicks w. tumors*	*Avg. tumor size*
4-Aminopteroylglutamic acid (Amino pterin)	10 γ/10 γ	0 (10*)	—	—
	10 γ/250 γ	6 (10*)	83	++
4-Aminopteroylglutamyl glycine	10 γ/10 γ	2 (10*)	0	0
	25 γ/10 γ	0 (10*)	—	—
4-Aminopteroylglutamyl β alanine	10 γ/10 γ	2 (10*)	0	0
	25 γ/10 γ	0 (10*)	—	—
4-Aminopteroyl d (—) glutamic acid	5 γ/20 γ	10	70	+++
	20 γ/20 γ	10	60	+++
4-Aminopteroylaspartic acid	100 γ/5 γ	10	0	0
	100 γ/10 γ	10	20	+
	100 γ/25 γ	9	33	+
4-Aminopteroylthreonine	10 γ/10 γ	10	50	++
	25 γ/10 γ	10	20	+
4-Aminopteroyl α alanine	10 γ/10 γ	10	70	++
	25 γ/10 γ	10	60	++
4-Aminopteroylserylglutamic acid	50 γ/20 γ	10	90	+++
	100 γ/20 γ	10	30	++
	200 γ/20 γ	10	30	+
	500 γ/20 γ	9	0	0
	1000 γ/20 γ	6	0	0

* Where toxicity of compounds reduced the number of birds in the group, (*) signifies the original numbers used.

TABLE 5

THE INHIBITORY EFFECT OF OTHER RELATED COMPOUNDS ON THE ACTIVITY OF PTEROYLGLUTAMIC ACID IN GROWTH OF ROUS SARCOMA

Compound	*Am't inj. daily/am't of PGA*	*Total no. of chicks*	*% chicks w. tumors*	*Avg. tumor size*
9,10-Dimethylpteroylglutamic acid	10 γ/10 γ	10	20	++
	100 γ/10 γ	9	22	+
	500 γ/10 γ	10	30	+
6-Methyl-7-pteroylglutamic acid	10 γ/10 γ	10	50	++
	100 γ/10 γ	9	77	++
	500 γ/10 γ	8	87	++
	1000 γ/20 γ	10	80	+++
7-Methylpteroylglutamic acid	10 γ/10 γ	10	50	++
	100 γ/10 γ	10	20	+
	500 γ/10 γ	9	22	+
7-Carboxy-6-pteroylglutamic acid	100 γ/20 γ	9	100	+++
	1000 γ/20 γ	9	88	+++
Pteroyl (d) serylglutamic acid	50 γ/20 γ	7	28	++
Controls	10	20	85	++
	20	10	100	+++

The inhibitors just mentioned are completely reversed by PGA alone. When vitamin B_{12} is given to chicks, however, the amount of PGA required for this reversal is markedly reduced. TABLE 3 gives a typical example. Vitamin B_{12} alone has little effect in supplementing the growth effect of PGA on the diet used, a typical synthetic diet containing 30 per cent protein (group 1–6). Chicks given 500γ of 4 aminopteroylaspartic acid failed to survive. A slight reversal occurred with 5γ of PGA and complete reversal was obtained with 25γ of PGA. In the group receiving 0.2γ of vitamin B_{12}, however, the inhibitor was reversed by only 5γ of PGA. In the absence of PGA, the B_{12} has no apparent effect. This experiment has been repeated with aminopterin and with 4 amino-9-methyl PGA. Similar results were obtained, using both vitamin B_{12} and 15 unit liver to obtain this lowering of the inhibition index.

The most potent inhibitor is aminopterin. As little as 20γ in a single dose will kill a weanling rat in a few days. The histological changes produced by this compound are evident within 24 hours. The bone marrow, thymus, and spleen are principally affected. Other derivatives produce similar changes at high levels.

The discovery that folic acid is essential to the growth of Rous sarcoma implants in chickens makes this tumor a useful tool in studying the role of nutrition in tumor development. Vitamin B_{12} has also been shown to be involved in the growth of this sarcoma.

The level of dietary protein markedly affects the growth of Rous tumor. FIGURE 1 illustrates the effect of several levels of dietary protein on the growth of the tumor in the absence of PGA and vitamin B_{12}. At protein levels below 30 per cent, a high incidence of tumors is obtained, while at levels of 30 per cent and over, the incidence drops to zero.

Further illustration of this is given in FIGURE 2. Chicks fed 30 per cent and 60 per cent protein require both vitamin B_{12} and PGA for maximum tumor development, while at the lower levels neither factor is required. It would appear that the requirement for both of these factors is increased with a high protein intake. Such chicks were found to be more sensitive to the antagonists. On protein-free diets, the chicks develop well-defined tumors even though the birds lose considerable weight.

The inhibition of Rous sarcoma by various PGA antagonists follows the pattern of growth inhibition. TABLES 4 and 5 give some typical examples, which need no further discussion here.

Histochemical studies on Rous tumor have demonstrated the presence of many substances stainable by various techniques. Preliminary studies on the effects of 4 aminopteroylaspartic acid on the tumor showed that phosphamidase, potassium, and iron were decreased, while the cytochrome oxidase, zinc, and polysaccharide stains gave variable changes.

Several vitamins are known to affect immune responses in animals. PGA deficiency in chicks inhibits agglutinin response to *E. typhosa*, *B. abortus*, and other organisms. The administration of PGA permits a high titer to develop. TABLE 6 gives a typical example, in which the vitamin was incorporated in the vaccine. This immune response is inhibited by the deficiency or by administration of an antagonist.

TABLE 6

EFFECT OF PTEROYLGLUTAMIC ACID INCORPORATED IN THE VACCINE ON THE AGGLUTININ RESPONSE OF CHICKS TO *B. abortus**

Am't PGA in each dose of vaccine	*Total am't injected*	*Total no. of chicks*	*Avg. titer of agglutinins*	*Range of agg. titers*
None	0 γ	36†	1:3.5	0:1:8
5 γ	30 γ	7	1:12.5	1:4–1:32
20 γ	120 γ	8	1:18.0	1:16–1:32
50 γ	300 γ	11	1:157.0	1:32–1:512

* Day-old chicks were placed on the pteroylglutamic acid-deficient diet. On the 8th. 10th, 12th, 15th, 17th, and 19th days, doses of vaccine containing 10 billion formalin-killed *B. abortus* organisms (0.5 ml) and pteroylglutamic acid as indicated were given by intramuscular injection and the birds were bled 6 days after the last treatment.

† Six tests.

Summary

Biological studies on a number of PGA derivatives are reported. Certain interrelationships between PGA derivatives and dietary protein and vitamin B_{12} have been illustrated.*

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* The compounds used were prepared by chemists at the Lederle Laboratories and Calco Chemical Company. The biological experiments were done in cooperation with P. A. Little, P. K. Roesch, H. B. Ritter, B. L. Hutchings, and A. R. Whitehill. Because of space imitations, many obvious and pertinent literature references have been omitted.

*SECTION OF PSYCHOLOGY**

THEMATIC APPERCEPTION: FAILURES AND THE DEFENSES†

By LEOPOLD BELLAK‡

In speaking of thematic apperception, we will restrict ourselves to a consideration of responses to the Thematic Apperception Test (TAT), the Make A Picture Story (MAPS), the Symonds' Picture Story Test, the Children's Apperception Test (CAT), and the Four Picture Test (FPT) (with which last the author's experience is extremely limited.) Some of the observations we wish to make, however, may hold true for the broader field of projective techniques, including the Rorschach.

Failures of Apperceptive Methods

Among clinical problems which arise in the methods of personality investigation, the most obvious and clear-cut of all difficulties are the failures of apperceptive methods. The subject has found scant, if any, consideration in the literature thus far, possibly owing to the honeymoon spirit currently prevailing in clinical psychology. Nevertheless, an investigation of these failures should be very important for the basic understanding of the dynamics of these methods and their limitations.§

By failure of an apperceptive method, we refer to: (a) an apparent lack or paucity of meaningful results; and (b) results which are apparently grossly inconsistent with incontrovertible clinical facts.

(*a*) *Apparent Lack of Meaningful Results.* The most obvious extreme of this type of failure exists when a patient refuses to give any responses at all. On that continuum, we also deal with too-brief responses or those of a barren, purely descriptive nature. It is obvious that a certain amount of voluntary cooperation by the subject is indispensable: if projective techniques are compared to X rays, it is necessary that the patient at least step behind the X-ray screen. This "cooperation" lies on a continuum of a relationship between tester and subject which has been described as "rapport" and was recently discussed by Joel[2] as "the interpersonal equation in projective methods." It resembles in essence the transference-countertransference problems of the psychoanalytic session.

The test situation and the examiner himself may be apperceptively distorted by the subject (as is most clearly seen in the paranoid) and, at the same time, attitudes of the examiner may influence the situation. Joel's point is well taken, that it should be helpful to have the examiner's reaction to the subject recorded as part of every protocol. If "establishing rapport" means that one puts the subject at ease, one must be aware of one's own involuntary attitudes. At the same time, it must be remembered that overfriendliness may also cause difficulties, such as homosexual anxiety. In this context, it should be pointed out that the influence of the sex of the examiner on the test results has never been sufficiently investigated.

Having touched briefly on the more obvious problems pertaining to lack or great dearth of responses, it remains to discuss those cases where the responses are quantitatively sufficient but barren in quality. This problem is so closely related to the situation where the results are inconsistent with known facts that we will discuss it in that context.

(*b*) *Results Inconsistent With Clinical Facts.* As an example of inconsistency of TAT results, we will cite the case of a 14-year-old boy who had killed a small child, apparently in some sexual excitement. Institutionalized, he was a model inmate on a behavioral level—bland, noncommittal, and inaccessible to psychotherapy. At the time of commitment and again when he came up for disposition at the end of a year, he was given Thematic Apperception

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* There was no meeting in January of the Section of Physics and Chemistry

† This paper, illustrated, was presented at the meeting of the Section on January 16, 1950

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§ A detailed discussion of specific cases is planned for the future by Mr. Ernest Parkes and the author.

Tests, Rorschachs, and other tests by the staff for help in understanding his problems. When none of the tests threw any light on the psychodynamics of the crime, he was referred to me for the administration of a TAT under the influence of intravenous sodium amytal. After the intravenous administration of $7\frac{1}{2}$ gr. sodium amytal and $1\frac{1}{2}$ gr. nembutal orally, a marked psychomotor effect of the drugs was noted: general relaxation, drowsiness, slurring of speech, and occasionally such lack of motor control and sleepiness that the TAT cards dropped out of his hands. Despite the action of the drugs, the boy gave 15 stories, which in no single instance revealed any *primary* pathological evidence in the theme itself that would have permitted one to pick out his TAT as belonging to a noticeably disturbed person.

There are a number of such cases and similar ones. For instance, in a woman patient of 30 with severe sadomasochistic problems and extensive phobias, which were studied in detail during two years of psychoanalysis, the TAT, administered prior to the beginning of analysis, failed to show any clear evidence of the existing pathology.

Needless to say, there are a great many protocols where a good deal of seemingly pathological material appears which cannot be correlated with the factual clinical data. I am not even referring to the vogue for diagnosing nonexistent schizophrenias. It seems that projective methods often reveal potentialities rather than actualities. The question we are faced with is: Just what happens in the kind of cases cited here? How does it happen that these patients' problems fail to appear in the tests, even though we have every reason to believe that the subjects are consciously cooperating to the best of their ability? We may profit by comparing these findings with a comparable psychoanalytic situation, namely, the one of unconscious resistance and a mobilization of defenses.

Defenses in Apperceptive Techniques

In clinical psychoanalysis, attention was first primarily focused on the "id" drives. Later developments, outstandingly Anna Freud's book, "The Ego and the Mechanisms of Defense,"[1] revealed the personality as a delicate matrix of the defense mechanisms and led to the study of the ego. Similarly, it can be said that the id impulses, needs, and drives have so far been the center of attention in apperceptive and, possibly, all projective techniques. The time has come to make a step comparable to the one in psychoanalytic development and study apperceptive protocols specifically from the standpoint of the defenses as indicators of personality structure, as criteria of assets and liabilities, and often as the only clues to impulses which themselves may have become completely invisible.

Defenses in the TAT can be observed in the story content itself, as in the story sequence, or in the sequence of themes to the same picture. For instance, simply not seeing the gun in 3 BM or 8 BM constitutes a scotoma, a simple repression. Similarly, if a patient referred to the woman leaning against the tree in #2 as slim, thin, and well proportioned, when most other people see her as presumably pregnant, we deal with direct denial and turning into the opposite. Displacement of a story into the middle ages or onto the moon, *etc.* is a familiar mechanism.

On the other hand, if we have a story to 17BM of a circus performer who always wanted to have a solo act instead of being part of a trio, and the story progresses to where another member of the troupe has an accident in which the hero is helpful and is rewarded by a solo act, then we can formulate the following: this person wants to be in the limelight and do away with the competitors; this thought is not permitted expression. Instead of the aggressive thought, the hero gives help in an accident; that is, the aggression is impersonalized, the helpfulness appears manifestly, and the original goal is given as a reward. In other words, sequence of the story demonstrates this subject's use of reaction formation in dealing with aggression. Sometimes a picture of the entire defensive nature of the character structure reveals itself clearly in the TAT.

On the other hand, one may see particularly obsessive compulsives offer several themes to one story, guarding against aggressive thoughts which nevertheless come out even in the final elaboration. However, we can differentiate patients who start with the most manifest appearance of the aggressive (or sexual) impulse and end with a most disguised theme to one picture from those patients who do it in reverse, *i.e.*, those whose first attempt at a story to a picture is the least manifest, with the impulse being permitted more and more direct expression as the patient tries to offer a different story. My assumption, so far, has been that in the first case there is still a firmer control of the forbidden impulse, which has to be interpreted in the context as either more anxiety or a relatively stronger ego.

It is clear that the response to a test situation constitutes a problem which the patient has to solve. The type of ego activity involved is fairly unique in that it is much more controlled than a nocturnal dream, involves a more defined task then the daydream, and needs freer reins than do the structured problems of everyday routine. Thus, the situation demands an ego that can perform a specific task and yet allow some relaxation of its grip on the id impulses (if such anthropomorphic language may be permitted). It must be borne in mind in this context that it is a normal function of the ego to *exclude itself.** If this function of self-exclusion of the ego is disturbed we see such disorders as insomnia, inability to rest, and inability to perform creative tasks. In these cases, we find that a relatively weak ego is so afraid of a break-through of forbidden id impulses that it dares not relax its constant vigil. Similarly, resistance in analysis occurs when there is an increase of anxiety concerning certain thoughts which the ego defends itself against by excluding *all* thoughts. Such extremes in characterological defensiveness are clinically well known as manifestations of rigidity and lack of spontaneity.

We may now apply what we know of defensiveness in the therapeutic situation to apperceptive techniques. It becomes easily intelligible that extreme defensiveness may be the reason for barren or inconsistently innocuous protocols in clinically very distorted patients.

Defensive "Failures" in Relation to General Problems of Interpretation

Rosenzweig[3] has recently pointed out that we deal with two types of frames of reference in the interpretation of apperceptive methods. The first concerns an interindividual comparison, *i.e.*, of a given subject's responses to the responses of a sample population. For instance, as in psychometric tests, we say that a large percentage of the population sees the pistol in 3 BM, and that therefore not seeing it must have a meaning. The second type of comparison we make is an intraindividual one. We study the repetitiveness of certain themes and base our interpretations on this factor. I would add to his conceptualization a third comparison as the basis of interpretation, namely, a comparison of the fantasy test behavior with the manifest personality.

With these frames of reference in mind, we can then state that, if a response is markedly different from those given by the population sample, *e.g.*, markedly more benign, we have reason to suspect it. For example, if the majority of subjects see the two figures in 18 GF as one strangling the other, and the young murderer mentioned earlier says that it is a mother consoling her daughter for having fallen downstairs, we can infer that this response is possibly motivated by his defense against his own aggression. On the other hand, if we find that a subject gives a theme of extreme aggression to a story and then changes it to a more innocuous version and later has primarily innocuous themes, we are again entitled to the inference that they are the result of defensiveness. Similarly, it is now obvious that projective protocols must be considered hand in hand with as much clinical information as possible for a comparison and mutual evaluation of the two sets of data.

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* We are indebted for this formulation to a verbal communication by Dr. Ernst Kris.

As in the psychoanalytic situation, small break-throughs of original impulses must be looked for to illuminate what it is the patient is resisting. For instance, in the stories of the young murderer, he made one slip of the tongue in his story to picture 8 BM, where he said,

> "These two boys have gone out hunting They shot a lot of boys—I mean, animals, and decided to split up to get more pheasants They agreed to meet at 8:30 at night to count the game and to go home. This boy was standing in the bushes Twenty feet from him he heard something move. He goes to look at what he caught and finds his friend and rushes him to the doctor The doctor says the boy will be all right and in the end the boy gets O.K. and forgives him for what he did."

The slip betrays the apparently original aggressive impulse which becomes absorbed in a perfectly acceptable hunting story, then breaks through again in the boy's injury. An inquiry into the small detail of 8:30 at night revealed that that was the time for lights out at the institution and strongly suggested that it is at this time that his fantasies are permitted to emerge. Similarly, he started his story to picture 1 like this:

> "This boy does not like to practise his trumpet—I mean he likes to play his violin."

He changes a negative response immediately to a positive one. The story he gives to picture 15 is as follows:

> "An undertaker; he is very sad because he is putting to rest all these people. He goes out one night and looks with remorse at all the work he has done. He tries to find out how he can undo his work. He can do nothing about it."

In this story, the undertaker behaves as if he had killed all the people he buried, revealing the original aggressive impulse only by what would otherwise be incongruent guilt feelings.

One must also make note of negations in TAT stories; that is, when a subject says that the hero is *not* angry, it presupposes that the first thought was that he *is* angry, and that the negation is defensive. Freud stated that such thoughts can best be translated "I wished he were not angry." By using such methods then, we trust that what may at first glance appear as barren and unrevealing stories, constituting failures of apperceptive techniques, may be turned into significant information, particularly concerning defenses.

To avoid barren records, sedation is usually more helpful, in my experience, than in the case of the young murderer. Either intravenous sodium amytal or oral sedation by fast-acting barbiturates are frequently effective means of decreasing the defensiveness in subjects in whom previous tests were relatively uninformative or in whom one has reason to expect unusually great conscious and unconscious resistance. The barbiturates presumably decrease the anxiety, and thus the need for defensiveness, and at the same time decrease the ego strength.

Another possible approach to avoiding relatively uninformative records is suggested by a recent experiment by Stein[4] on the effect of time pressure on Rorschach results. He exposed Rorschach cards tachistoscopically, at four different speeds, and found an increasingly clearer revelation of the "deeper" problems, less and less disguised by defenses. One might draw the inference from this experiment that in certain subjects, where one need not worry too much about the deleterious effect of additional anxiety, a time limit on apperceptive performances may produce better results. The anxiety may be so increased that the defenses break down. On the other hand, in some patients, this may lead to further tightening up and complete refusal of responses.

Summary

Failures of apperceptive techniques, as one problem clinicians face, have to be understood in terms of defensiveness against anxiety, as in the psychoanalytic situation.

A study of secondary features of the protocol may reveal the nature of the defenses, and indirectly the warded-off impulses. Pre-test sedation and time limits for responses may be two means of avoiding relative lack of information by decreasing anxiety or ego strength in turn.[*]

[*] After presentation of the paper, the author received a paper by Dr. Silvan S. Tomkins, "The present status of the thematic apperception test" (from the *American Journal of Orthopsychiatry*, Vol. XIX, No. 2, April 1949), which also deals with the problem of failures with the TAT.

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SECTION OF ANTHROPOLOGY

THE MEXICAN CALENDAR AND THE FOUNDING OF TENOCHTITLAN-TLATELOLCO[†]

By PAUL KIRCHHOFF[‡]

Mesoamerica is the only part of pre-Columbian America where an advanced calendar and pictorial writing created the basis for the keeping of historical records. In this region, therefore, and in this region alone, can the study of the archaeological remains be supplemented by the study of native annals, thus creating a situation similar to that found in many parts of the Old World. The combination of these two types of research should give us a much better knowledge of the actual course of events in this part of pre-Columbian America than we have now, and, based on this better knowledge, a better understanding of historical processes and forces involved than we may hope to attain in any other part of America.

One of the most important and interesting social phenomena in Mesoamerica, paralleled in the New World only in the Andean civilizations, is the city. The history of Mexico City is unique in that it is the only well-documented history of a pre-Columbian city anywhere on this continent covering a period of considerable length. This history, which has not yet been studied properly, contains a great wealth of important data, among them the dates when certain public buildings (pyramids, temples, and aqueducts) were started and finished.

As a preliminary report on an investigation still in progress, I have selected for analysis one single moment in the history of Mexico City, that of the founding of the twin cities, Tenochtitlan and Tlatelolco, of which it consisted. Out of the many interesting aspects of this event that could be studied, I have selected the one that may seem the most arid but which actually is the indispensable basis and starting point for the study of all other phases, as well as for the study of the subsequent history of Mexico City, namely, the chronological aspect. The exact date of the founding of Tenochtitlan and Tlatelolco is important above all because we want to know how long it took to build the city the Spaniards found and described. This will be of importance not only for the history of Mexico City itself, but also because it will provide us with a comparative yardstick for the study of the archaeological remains of other cities for which no written records exist and, at the same time, for our comprehension of the nature of Mesoamerican cities and of Mesoamerican society as a whole.

[†] This paper was presented at the meeting of the Section on January 23, 1950. The research for this paper was supported by a grant-in-aid from the Viking Fund, Inc.

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For that part of Mesoamerica with which we are here concerned (the Valley of Mexico and surrounding regions), the principal features of the calendar serving as a basis for dated recordings may be summarized as follows. Every day and every year has a name, composed of a number (1–13) and a sign (20 in the case of day names, but only 4, called year bearers, in the case of year names). The resultant combinations of numerals and signs are 260 in the case of day names and 52 in the case of year names. After 260 days and after 52 years, new day and year cycles start, with day names and year names identical with those of the preceding periods.

TABLE 1

Dates	*Founding of*	*Sources*
(1) 1 Rabbit =	1194: Tenochtitlan	Codex Xolotl, Alva Ixtlilxochitl
cf. 1 Rabbit =	1246: conflict with Colhua; the Mexicans leave Colhuacan	Anales de Quauhtitlan .
(2) 2 House =	1273: Tlalcocomocco (Tenochtitlan)	Anales de Quauhtitlan
(3) 8 Rabbit =	1318: Tenochtitlan	Anales de Quauhtitlan (Duran says only: "1318")
(4) 2 House = 1324	1325: Tenochtitlan, Tlatelolco	(Tenochtitlan, 1324:) Codex Mendoza, Mendieta, *etc.*; (Tenochtitlan, 1325:) Anales de Tlatelolco, Chimalpahin, *etc.*; (Tlatelolco, 1325:) Veytia
(5) 3 Rabbit =	1326: Tenochtitlan	Tezozomoc, according to Veytia (doubtful)*
(6) 4 Reed =	1327: Tenochtitlan, Tlatelolco	(Tenochtitlan and Tlatelolco:) Tablas cronologicas in Boturini-Veytia's "Texcoco en los ultimos teimpos"; (Tenochtitlan:) Sigüenza, and authors following him
(7) 9 Knife = 1331†	: Tenochtitlan	Ventura Zapata, according to Veytia
(8) 1 House =	1337: Tlatelolco	Anales de Tlatelolco, Chimalpahin, *etc.*
(9) 2 Rabbit =	1338: Tlatelolco	Clavigero
(10) 2 Knife =	1364: Tenochtitlan	Codex Aubin
(11) 4 Rabbit =	1366: Tenochtitlan	Codex Vaticanus A, Mappe de Tepechpan

* The inclusion of this date is based only on the statement by Veytia that "Alvarado Tezozomoc gives to understand that the year was 3 Rabbit, which could refer to the year 1326." No such reference can be found in Tezozomoc's published writings, but it is quite possible that Veytia used another manuscript of Tezozomoc's that has been lost since.

† Veytia's "1321" is clearly a misprint, as the context shows.

In the original sources, lost today, events seem to have been recorded either under a year name only or under a year name and a day name. The secondary sources known at the present time, whether pictorial or written, contain very few day dates, a minute fraction of the great number of year dates recorded. Sources written in the Nahuatl language give their dates in terms of the native calendar, and fortunately some of the sources written in a European language do the same. Others, however, give only the equivalent dates in our own calendar. Thus, the only day date referring to the founding of Mexico City, recorded by Sigüenza y Gongora in the 17th century, on the basis of a pictorial history since lost, is given only in terms of our calendar—July 18. The day date 1 Crocodile, recorded in the "Anales de Tlatelolco," seems to refer to a somewhat later moment, namely, the inauguration of the first, still very simple, pyramid in Tenochtitlan.

The retranslation of dates given only in terms of our calendar into the native calendar, on the assumption that the most common correlation, 1519 = 1 Reed, was used, has proved feasible and useful in other cases. We have refrained, however, from the inclusion of such retranslated dates in our analysis of the dates given for the founding of Mexico City.

TABLE 1 lists all known native dates for the founding of either Tenochtitlan or

Tlatelolco, the twin cities founded by the Mexicans, and the years of our calendar with which these native years have been equated by our sources, explicitly or implicitly. Equations based on the correlation 1519 = 1 Reed have been entered in the second column, those based on the less frequent, but by no means uncommon, correlation 1518 = 1 Reed (*i.e.*, 1519 = 2 Knife), in the first.

It is often said that the difficulty with Central Mexican annals is that there is no way of distinguishing the years of the same name in different 52-year cycles. This is obviously not the source of our difficulty in this case, as it is in all but a few others. Only one date, that of 2 House, occurs twice in our list. A much greater difficulty lies in the curious fact that, whereas all other dates are given for the founding of either Tenochtitlan or Tlatelolco, 2 House and 4 Reed are given for both. Taking this into consideration, we are faced, then, with eight different native dates for Tenochtitlan (counting the two 2 House dates as only one) and four for Tlatelolco. Some of the dates for Tenochtitlan precede those for Tlatelolco, while others follow them.

TABLE 2

(1)	8 Rabbit	=1318:	Tenochtitlan	
(2)	2 House	=1325:	Tenochtitlan,	Tlatelolco
(3)	3 Rabbit	=1326:	Tenochtitlan	
(4)	4 Reed	=1327:	Tenochtitlan,	Tlatelolco
(5)	9 Knife	=1332:	Tenochtitlan	
(6)	1 House	=1337:		Tlatelolco
(7)	2 Rabbit	=1338:		Tlatelolco
(8)	1 Rabbit	=1350:	Tenochtitlan	
(9)	2 Knife	=1364:	Tenochtitlan	
(10)	4 Rabbit	=1366:	Tenochtitlan	

The first two equations with years of our calendar are undoubtedly far too early. Mexico City was certainly founded sometime in the fourteenth century. By adding a full 52-year cycle to the second date in our list (*i.e.*, the first 2 House date), we arrive at the year 1325, the year with which the second 2 House date has been identified by other sources; and by adding three complete 52-year cycles to the first date in our list, we arrive at another 1 Rabbit date, namely, 1350, which seems more in line with the other equations. These changes result in the reformed TABLE 2, in which, for simplicity's sake, we have also converted all equations based on the correlation 1 Reed = 1518 into those based on 1 Reed = 1519. This is the chart which will be used in the following analysis.

While we have now reduced the total time span, in terms of equations with our calendar, from 172 years to 48 years, we are still confronted with essentially the same conflict between a great number of dates, and of years over which they spread, and the clear indications in all our sources, sometimes explicit, sometimes implicit, that the founding of both Tenochtitlan and Tlatelolco were events which took place in specific years, and in those years alone. We might, in spite of this, feel inclined to interpret the repeated spreading of dates over a short number of consecutive years (for Tenochtitlan, twice over three years, and for Tlatelolco, once over three years and once over two years), as indicating, after all, a slow settlement that spread over a number of years, if it were not for the fact that for each city there are *two* such blocks of years. Besides, we find the same spread over as many as four consecutive years (though never more) to be characteristic of all kinds of events, including those which, like coronation ceremonies or solar eclipses, took place on a specific day. These regular differences, not fully understood at the present time, may be connected with different year beginnings, possibly each of them characteristic of one of the four quarters into which so many Mesoamerican cities were divided.

More striking is the great number of dates that are widely separated. In view of the obvious impossibility of reconciling them, so long as modern students have accepted the tacit assumption of sixteenth century writers that all these dates belong to one single year count, and in view of the equal impossibility of accepting all of them, the only feasible, though very unsatisfactory, procedure seemed to be to select one, or rather two, as "the most acceptable." Most modern authors have selected the date 2 House = 1325 for the founding of Tenochtitlan, and the date 1 House = 1337 (*i.e.*, 12 years later) for the founding of Tlatelolco; and this in spite of the fact that several sources definitely state, or at least imply, that not Tenochtitlan but Tlatelolco was founded first.

The division of the Mexicans into two sections, which settled apart and started independent histories with opposite foreign orientations—Tenochtitlan towards Colhuacan, and Tlatelolco towards Azcapotzalco (see FIGURE 1)—is an event of first-rate significance, and it would be important to be able to decide whether there was for a number of years a common settlement from which, several years later, one group split away, or whether the two groups started separate settlements from the very beginning, as Boturini and the author of the "Historia de los mexicanos por sus pinturas" seem to imply. It is significant in this respect that Veytia, according to whom the founding of Tlatelolco preceded that of Tenochtitlan by two years, gives the date 2 House, usually connected with Tenochtitlan, for the founding of Tlatelolco.

Recent research has led to discoveries which make possible a new approach to our problem. It has been established that 13 different year counts existed in the area from the Mesa Central to Northern Oaxaca. This number includes the year count which for a long time had been considered to be the only one used in that area, *i.e.*, the year count of Mexico City, in which the year 1519 was called 1 Reed; the Mixtec-Popoloca year count discovered by Jimenez Moreno, in which that year was called 13 Reed; and the Matlatzinca year count established by Caso, in which it was called 3 Reed. The additional ten year counts that have been discovered since follow the same pattern. In each of them, the same year has the same year bearer but a different numeral. With 13 numerals used, 13 different year counts are possible, and all of them are found to have actually been in use.[*]

The utilization of the 13 year count correlation in the analysis of apparently conflicting dates for a given event has opened up new avenues of research. No longer do we have to select from several dates one as "the most acceptable." Every one of them turns out to be correct, though only within a given year count used by a particular people or in a particular city. Far from leaving us in a state of bewilderment, as before, these apparently conflicting, but in reality parallel, dates now help us to place an event with an accuracy that increases with each additional date that we discover in our sources.

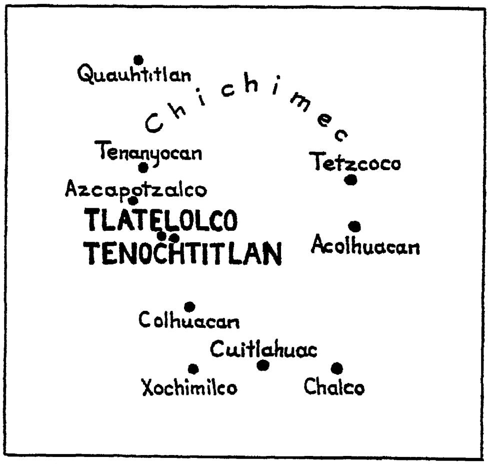

FIGURE 1. Principal cities in the Valley of Mexico

This is as true of the many dates reported for the founding of Mexico City as it is of all other dates that we have analyzed in the course of our investigations. The 13 year count correlation, however, has not been worked out on the basis of the analysis of such individual events. It is based rather on analysis of coupled events, for which several sources give different dates but equal intervals, and on specific statements in our sources to the effect that certain dates belong to the historical records of certain peoples or cities. Our dates for the founding of Mexico City are unsatisfactory in both respects. The situation is especially difficult, as well as intriguing, because practically all of our sources switch from one year count to another, at the time of the founding of Mexico City or of one or both of the important events preceding or following it: the arrival and defeat in Chapoltepec, and the crowning of the first two kings of the newly founded twin cities. It is apparent already that these shifts from one year count to another are related to realignments among the peoples living in the Valley of Mexico at that time. To date, however, only the barest beginning has been made along this line of analysis.

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[*] The spread of dates referring to a single event, over up to four consecutive years, seems to occur in several, and possibly all, of the 13 year counts This suggests the possibility that each of them had several variants, sometimes, if not always, four in number.

TABLE 3

SEGMENTS OF SIX YEAR COUNTS INTO WHICH FIT THE DATES REPORTED FOR THE FOUNDING OF TENOCHTITLAN AND TLATELOLCO, AND THE CHRISTIAN YEARS TO WHICH THEY CORRESPOND

Year counts of						
Colhuacan	*Cuitlahuac*	*Azcapotzalco*	*Chichimec*	*Huexotzinco? Tlaxcallan?*	*Mexico City*	*A.D.*
2 KNIFE*	1 Knife	13 Knife	12 Knife	9 KNIFE	6 Knife	1368
3 House	2 HOUSE	1 HOUSE	13 House	10 House	7 House	1369
4 RABBIT	3 RABBIT	2 RABBIT	1 RABBIT	11 Rabbit	8 RABBIT	1370
5 Reed	4 REED	3 Reed	2 Reed	12 Reed	9 Reed	1371

* Capital letters are used for those year names for which this event has been reported.

Applying the 13 year count correlation (segments of which are shown in TABLE 2) to the dates referring to the founding of Mexico City, we find that all of them (*i.e.*, both those reported for Tenochtitlan and those for Tlatelolco) correspond to the same year, or rather to a group of four consecutive years (see above). Tenochtitlan and Tlatelolco must have been founded in the course of the same year, and not 12 years apart.

Our dates for the founding of Tenochtitlan come from five different year counts: those of Mexico City itself, Colhuacan, Cuitlahuac, the Chichimec of Tenanyocan-Tetzcoco-Quauhtitlan (all in the Valley of Mexico—see FIGURE 1), and either Huexotzinco or Tlaxcallan (in the Valley of Puebla). The dates for the founding of Tlatelolco come from only two year counts: again, that of Cuitlahuac, and that of the Tepanec of Azcapotzalco.

While not too much significance should be attached to the fact that dates for this event are not available in all of the year counts of the nearest neighbors of the newly settled Mexicans, it is certainly striking and suggestive that dates from Colhuacan (*i.e.*, the city that gave Tenochtitlan its first king) are known only for Tenochtitlan, and dates from Azcapotzalco, the city that gave Tlatelolco its first king, for Tlatelolco alone. On the other hand, two dates from the Cuitlahuac count are reported for the founding of both Tenochtitlan and Tlatelolco. What is still more striking, precisely those sources that give an Azcapotzalco date for the founding of Tlatelolco, give for the founding of Tenochtitlan not a Colhuacan but a Cuitlahuac date, namely, the year 2 House. Similarly, Chimalpahin, an author from Chalco who uses the Chalco year count for the earlier history of his city, uses the Cuitlahuac, not the Mexico City year count, for the earlier history of the Mexicans, including the Cuitlahuac date 2 House for the founding of Tenochtitlan. Another example is the "Histoyre du Mechique," a source based largely on the tradition of Acolhuacan, which likewise gives this Cuitlahuac date, although interestingly enough it equates it with the Christian year 1321 (instead of the usual 1325), which shows that its author used an Acolhuacan correlation chart.

As a matter of fact, the Cuitlahuac date for the founding of Tenochtitlan (and similarly, the earlier Cuitlahuac date for the defeat in Chapoltepec) seem to have been considered by one Mexican tradition, represented by several important sources, as actually belonging to the year count of Mexico City, just as the Azcapotzalco date for the founding of Tlatelolco. It is not clear yet what gave the city of Cuitlahuac, or "Little Venice" as it was called by the Spaniards, such an outstanding place in the history of the people who settled in Mexico City; but the general importance of that city, scarcely ever mentioned by modern students, is beyond any doubt. The importance of Cuitlahuac dates for the founding of Tenochtitlan is further underlined by the fact that the only two day dates known today likewise come from the Cuitlahuac calendar.

Only one of the dates for the founding of Tenochtitlan, namely, 8 Rabbit, comes from the year count of Mexico City, and interestingly enough it is reported by only one source and with but the merest mention of the event, without any of the usual details, "This is the year in which Mexico-Tenochtitlan started."

The date 9 knife, reported by a Tlaxcaltecan source, belongs to a year count which we have tentatively identified with either Huexotzinco or Tlaxcallan. The date 1 Rabbit, given in the "Codex Xolotl" and by Alva Ixtilxochitl, both Tetzcocan sources, belongs to the year count that was used by the Chichimec of Tenanyocan-Tetzcoco-Quauhtitlan. The anonymous author of the "Anales de Quauhtitlan," who explicitly states that he makes use of the annals of several peoples or cities, with conflicting dates, reports for a year 1 Rabbit a fight between the Colhua and the Mexicans and the withdrawal of the latter. The context makes it clear that this is the battle that took place at the time the Mexicans abandoned Colhuacan, where they had lived in vassalage and from where they proceeded to settle in Tenochtitlan and Tlatelolco. Our source reports this fight in a section which begins with the words "This is the story of the old men of Quauhtitlan," and this date actually belongs to the year count which we have been able to identify with the Chichimec of Quauhtitlan, as well as of Tenanyocan-Tetzcoco.

Actually, the "Anales de Quauhtitlan" gives three different dates for the founding of Mexico: (1) the date 1 Rabbit, equated with the year 1246, for the Mexicans' battle with the Colhua and their departure from Colhuacan; (2) the date 2 House, equated with the year 1273, for their settling in "Tlalcocomocco," which is only another name for the locality where Tenochtitlan was founded; and (3) 8 Rabbit, equated with the year 1318, for the "beginning in Mexico-Tenochtitlan." Since the author of the "Anales de Quauhtitlan" did not realize that these are three parallel and synchronous dates, belonging to different year counts, he erroneously accommodated them all in one count. As a result, his whole chronology was hopelessly thrown out of gear and tremendously lengthened.

Since out of the total number of ten dates only one, 8 Rabbit, belongs to the year count in which 1 Reed corresponded to the year 1519, it is obvious that we have to disregard all other proposed equations with years of our calendar, as well as all statements in our sources as to the number of years by which the founding of Tenochtitlan is said to have preceded the crowning of its first king (statements that vary all the way from 10 years, in the "Mappe de Tepechpan," to 104 years, in the "Relacion de la Genealogia *etc.*"). The equation of the date 8 Rabbit with the year 1318 is, however, likewise unacceptable, since it would leave us with a full 52-year cycle for which nothing whatsoever is reported (and this immediately after and before years which are known to have been crowded with events) and since in addition it would oblige us to add 52 years to the lives and reigns of individuals who, like king Tezozomoc, are mentioned both before and after this date. We therefore equate the date 8 Rabbit not with the year 1318 but with 1370, that is, a full 52-year cycle later.

On the basis of the 13 year count correlation chart, this equation gives us the correct equations of all other dates analyzed here. They correspond to the years 1368, 1369, 1370, and 1371. As long as this four-year spread, characteristic of so many dates, is not fully understood, we propose to select provisionally the two middle years, and consider 1369 to 1370 as the date on which both Tenochtitlan and Tlatelolco were founded. This gives us a total time span of 150 years for the history of Mexico City from its beginning to the coming of the Spaniards, as against almost 200 years, on the basis of the traditional date of 1325 for the founding of Tenochtitlan.

DIVISION OF MYCOLOGY

THE RELATIONSHIP OF *IN VITRO* TESTS OF ANTIFUNGAL SUBSTANCES TO THERAPEUTIC PROBLEMS[*]

By KURT A OSTER AND MILTON J. GOLDEN[†]

An enumeration of the therapeutic problems encountered in the treatment of tinea pedis shows them to be basically the same as those met in any infectious disease. They range from prevention of the spread of infection as a general health measure to the treatment of the individual case, from the problem of carriers to the question of individual hypersusceptibility to the causative organisms. The status of the actual therapy of these tinea pedis infections is still unsettled. Eradication of the infection is the claim of a multitude of recommended preparations.

A perusal of the various textbooks of dermatology brings forth the almost uniformly expressed opinion that dermatophytosis should not be treated with strong fungicides. The recommendations for therapy vary considerably, but, either by implication or by direct mention, the use of strong fungicides is a therapeutic taboo.

This report challenges such a statement. The supporting data have been derived in the greater part from *in vitro* studies of antifungal substances and only to a minor degree from clinical observations. This procedure, unusual in that the reverse is generally the case, led to the use of the rather ambitious title of this paper.

Theoretical Considerations. It is common usage in the treatment of any infectious disease (and dermatophytosis does fall into this classification) to give at the start of the diagnosed or presumed infection the proper chemotherapy, proper as far as choice of the drug and its dosage are concerned. Neglect of this maxim would lead to dire consequences. Spread of the infection to the surrounding tissues or to the blood stream, chronicity of the infection, and dormancy of the invading organisms, with later recurrences and eventual possible resistance of the organisms to the therapy, might be the disastrous results. Taking into consideration these well-established principles of treatment of infections by powerful drugs, the sentence in the textbooks should then read, "Dermatophytosis should be treated with strong fungicides, if we have them at our disposal." Unfortunately, there is available not one compound which can be rightfully classified as a strong (*i.e.*, effective and powerful) fungicide.

Definition of a Fungicide. This statement introduces a highly confusing controversy as to the definition of the term "fungicide." Most compounds used at present in the therapy of dermatophytosis are either keratolytic or fungistatic in their actions, but not fungicidal. Hopkins and collaborators, in their extensive clinical study, came to a similar conclusion.[1] An ideal fungicide is a compound which kills fungi in every stage of growth *in vitro* and *in vivo* on short contact and in relatively low concentrations without causing damage to the host during and subsequent to its therapeutic performance. To determine a compound to be a fungicide, one needs a proper fungicidal test designed to give this information exclusively and unequivocally. *In vitro* studies must pioneer its testing, since *in vivo* tests have not been extremely reliable thus far. Demonstrations on infected humans should follow to establish the clinical effectiveness and possible eventual skin toxicity of the drug on trial.

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[*] This paper, No. VII of a series entitled "Studies on Alcohol-Soluble Fungistatic and Fungicidal Compounds" and illustrated with lantern slides, was presented at the Meeting of the Division on January 27, 1950.

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Fungicidal Testing. Compounds often test wonderfully *in vitro* but show very little or no effect *in vivo*. The only lesson one should draw from this statement is that the *in vitro* tests were not properly designed to encompass the difficulties encountered in practical application. The fault lies with the tester and with the test, never with the test substance. Disregarding unknown personal sensitivities, the trichophytons grow on dead and macerated skin exclusively and therefore constitute an almost ideal example for chemotherapeutic endeavor, since healing here is the sole responsibility of the therapeutic substance. The body's own defense mechanisms, antibody formation, phagocytosis, *etc.*, would not come into play here as it does in combatting many bacterial infections.

Considering that eradication of the infection is almost solely a function of the fungicidal substance, a proper fungicidal test should also supply the answer to the question of the correct concentration of the chemical to be used in actual therapy. Most so-called "fungicidal" tests fall far short of this quality. The fungicidal dilutions of the test and the therapeutic concentrations in actual use differ frequently in multiples as high as a thousand times. By employing a functionally designed fungicidal test, it was possible to demonstrate the relative weakness of ten of the most frequently used antifungal substances.[2] This test was modified to determine also the fungicidal properties of antifungal ointments.[3] Most of these were also found to be either only fungistatic or possessing only extremely weak fungicidal properties. Use of this test method greatly diminished the discrepancies between *in vivo* and *in vitro* performances. The test is a manifestation of the relationship between a controlled contact time of fungus and toxicant, established by a suitable washing step, and the concentration of the drug. In this way, the first part of the challenge to the rule "Do not use strong fungicides" is brought out; we do not have any, as has been shown by these tests. Those substances which claim to be strong fungicides are really weak in performance and must therefore be used in high concentrations, which in turn might be irritating to the skin. Similar ideas have been brought out by Robinson for *Microsporon audouini*.[4]

Development of Cross Resistance. It is a commonly known fact that when bacterial infections are treated with suboptimal doses the invading organisms may develop a resistance to the drug in question. The infection must then be treated with far greater amounts of the chemical or it will no longer respond to the same substance, the organisms having become drug fast. The time element under which this bacterial resistance is developed is of major importance in therapeutic considerations. Furthermore, the organisms may or may not develop a resistance to similar therapeutic compounds chemically related or unrelated to the drug causing the resistance in the first place.

Application of this knowledge to the treatment of dermatophytosis is thus far deplorably lacking, as a survey of the pertinent literature will convince an interested investigator. Murphy and Rothman have reported that they succeeded in developing a strain of *T. mentagrophytes* with a sevenfold resistance to pelargonic acid.[5] It took them close to two years to obtain this particular strain, a period admittedly too long to be considered of importance in practical therapy of dermatophytosis. On the other hand, if it could be demonstrated that a measurable resistance of the fungi would develop to subfungistatic dosages of antifungal compounds in the relatively short period of one week, such findings would have a definite bearing on therapeutic problems and would constitute a second challenge to the statement, "Do not treat fungus infections with strong fungicides."

By growing *T. mentagrophytes* (640 Emmons) in the presence of undecylenic acid diluted 1:35,000 and 8-hydroxyquinoline benzoate 1:90,000 for one week only, in Sabouraud's maltose broth adjusted to a *p*H of 5.7, a definite increase in resistance could be demonstrated (TABLE 1). This increase in resistance was found by incorporation of the chemical into Sabouraud's maltose agar in various concentrations with subsequent streaking of the plates with the pretreated strain, which was kept growing as a stock culture on solid untreated agar and used after a period of 15 days. Using both strains interchangeably, it could be shown that a cross resistance* developed, as it was tentatively termed, of the trichophytons to a phenolic compound after exposure to a fatty acid on one hand and to a fatty acid after exposure to a phenolic compound on the other hand. These results are of only a preliminary nature at present. An investigation of the resistance behavior of the fungi to several other antifungal substances is under way, but the preliminary results appeared of such importance that they are included in this report dealing with the relationship of *in vitro* studies to therapeutic problems. It may be permissible at this point to speculate about the therapeutic significance of this finding.

TABLE 1

DEVELOPMENT OF CROSS RESISTANCE OF A FUNGUS CULTURE FOR UNDECYLENIC ACID AND 8,HYDROXYQUINOLINE BENZOATE AFTER PRE-TREATMENT WITH EITHER ONE OF THESE COMPOUNDS

	Original culture	*Culture A* undecylenic acid treated*	*Culture B* 8, hydroxy-quinoline treated*
Undecylenic acid dilutions:			
1:17500	+	+	+
1:15000	−	+	+
1:12500	−	+	+
1:10000	−	−	−
8,hydroxy-quinoline dilutions			
1:90000	+	+	+
1:70000	−	+	+
1:50000	−	+	+
1:25000	−	−	−

+ growth, − no growth.

* *T mentagrophytes* (No. 640 Emmons) grown for one week in broth containing: A Undecylenic acid dilution 1 35000, B 8, hydroxyquinoline dilution 1 90000.

By treating fungus infections with weak antifungal substances, frequently in suboptimal doses to avoid so-called "irritation," one might cause a resistance to the chemical in question to be developed by the infecting organisms. This resistance might then necessitate the use of larger concentrations of the antifungal substance to overcome the infection. If these prove unsuccessful or too irritating, an acute infection may then turn into a chronic one. (Unfortunately a multitude of tinea pedis cases are of a chronic nature.) At this point, the hypothesis is ventured that many chronic cases are the result of the insufficient treatment given at the beginning of the disease, due to the fact that the drugs used are either too weak or are used in suboptimal doses. Rothman, in his hypothesis about specificity of fungus infections of various body regions, sets up 3 points which would prove the validity of his postulation for tinea pedis.[6]

To prove the validity of the idea that fungi would develop a resistance in a short time when treated with suboptimal doses of antifungal substances not only *in vitro* but also *in vivo*, an additional point besides those mentioned by Rothman should be included. Fungi cultured from foot lesions before treatment should show less resistance to the antifungal substance used in the treatment than fungi taken from the same lesions after a short period of treatment. Experiments to examine this effect are in progress. That a positive answer to this question might be obtained seems to be indicated by the different courses of healing which a chronic fungus infection may take when treated with a rather weak and with a rather strong fungicidal substance.

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* Resistance in this paper is used only in the meaning of larger demands of substances in the test to accomplish fungistasis and does not imply, for the time being, the development of resistant strains or mutations.

The Course of Healing. The healing of tinea pedis infection is usually accompanied by considerable shedding of skin. Keratolytics are frequently used to promote this process. The so-called "soothing" applications for the treatment of the irritated skin will not be considered in this report, since they serve a different purpose. The antifungal substances are applied then for the actual eradication of the fungus infection. The present multitude of products and the variety of their concentrations and combinations of ingredients makes one wonder about their acclaimed effectiveness. Will they actually eliminate the last vestiges of the infection, or will they give only a patched-up performance by driving the infection underground into a quiescent state? The latter possibility would explain the frequent recurrences one encounters at seasonal intervals with chronic dermatophytosis. It would be a strange coincidence indeed that the so-called "reinfections" appear on the exact site of the original infection (*e.g.*, the plantar fold of the fifth metatarsophalangeal joint) and not in a new area. One would have to ascribe to the infecting fungi a precise aiming power or to the infected area a hypersusceptibility. Neither assumption sounds credible.

A more plausible explanation of the recurrence of fungus infections at their original locations would be the fact, so emphasized in this report, that the antifungal substances employed were simply too weak to eradicate the infection completely. At the advent of favorable growth conditions, the invaders started to multiply and to cause again a manifest infection. Most cases of so-called "reinfection" could then be classified as recurrences. The blame should not be placed on the usually innocent socks and shoes, but rather on the weak therapeutic compound. A scheme is presented in FIGURE 1 which attempts to describe roughly the differences encountered in the healing process with the use of a fungistatic substance and with an ideal fungicide.

It is assumed that there is a manifest infection present at the plantar fold of the fourth metatarsophalangeal joint caused by fungi producing a pathologic host-parasite reaction, surrounded by fungi existing only as parasites without causing an inflammatory state. The same condition could also be present at a plantar area near the instep of the sole (FIGURE 1 (1)). Treatment with a fungistatic compound would transfer the active fungi into an inactive state with subsequent disappearance of the host reaction and apparent healing, but with sufficient numbers of fungi left in a quiescent state to cause a recurrence at the identical location, if and when favorable growth conditions reoccur (FIGURE 1 (2)). The infection then is merely subdued, and, provided the fungistatic compound is not a primary irritant or skin sensitizer, little if any irritation would be noticeable, excepting some desquamation of the infected skin.

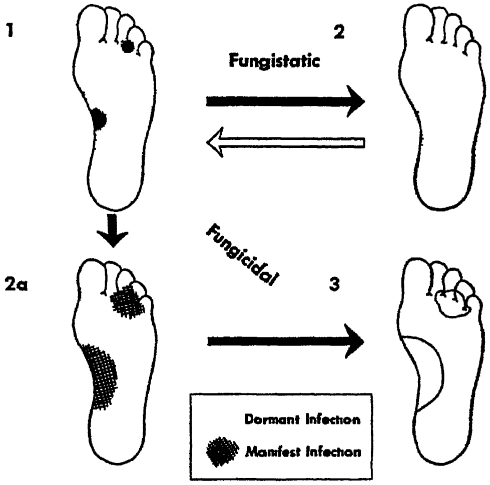

FIGURE 1 Scheme of healing of tinea pedis with fungistatic and fungicidal agents

Quite a different healing process would be encountered with a truly fungicidal substance. Its inherent effectiveness would activate or, if one may use the term, "irritate" the parasitic or dormant sites of infection. As a concentrated tuberculin injection may activate dormant foci, so the toxins of the killed fungi may activate dormant areas of fungus infection, which were not previously visible to the observer as active lesions (FIGURE 1 (2a)). A spurious spread of the infection might then be noted, giving in some cases the erroneous impression of a primary irritation by the fungicide itself. In many clinical reports this expression of irritation is presented but not defined or explained. In the same reports, there is usually no mention made of the number of relapses encountered after clinical clearing with fungistatic compounds. After the apparent spread of the fungus infection to other areas primarily not involved, the ideal fungicide would then eradicate the infection completely (FIGURE 1 (3)). Relapses in this case would then be called true reinfections. Spreading of the skin lesion may then be considered an essential part of the healing process of tinea pedis, just as there are more or less definite healing patterns for other infections, *e.g.*, pneumonia, exanthematous diseases, *etc.*

One of the most insistent objections to the use of strong fungicides is the argument that the sudden release of fungus toxins may precipitate or aggravate severe trichophytid reactions. The similarity of the Jarisch-Herxheimer reaction encountered in the treatment of lues to the occurrence of severe trichophytid reactions in tinea pedis is quoted as a realistic deterrent to the use of strong fungicides. It might be stated here, however, that the knowledge of the possible occurrence of this reaction in syphilis therapy when involving the coronary ostia, even with eventual fatal results, has not been and will not be a deterrent to the use of the chemotherapeutic drugs employed in the cure of this disease. The trichophytid reactions encountered in the treatment of tinea pedis may range from annoying to severe. One can express reasonable doubt that they will ever be fatal. Their incidence is not of such scope that they should constitute a serious objection to the use of an effective fungicide. Furthermore, with sound clinical judgment, the fungicide may always be diluted in such a way as to prevent the occurrence of too stormy a trichophytid reaction, if suspicion of such a happening might be aroused. This procedure is utilized with the arsenicals or penicillin in the treatment of lues. The occasional occurrence of trichophytid reactions of more serious nature should then constitute no objection to the use of a strong fungicide in tinea pedis therapy.

Conclusions. Dermatophytosis constitutes an infectious disease and should be treated according to the same principles governing the established treatment of other infections. These would include the use of strong and effective fungicidal substances, if these are available. Neglect of these therapeutic ideas may lead to the development of resistant fungus strains and to chronicity of the infection. These suggestions are in sharp contrast with the prevailing concepts of antifungal therapy.

Summary. The statement that dermatophytosis should not be treated with strong fungicides is challenged because of findings obtained for the most part from *in vitro* studies. Firstly, antifungal substances used in current therapy are merely fungistatic, but not fungicidal. Secondly, the fungi may develop a resistance to suboptimal dosages of fungistatic substances. The general principle of treating an infection with optimal doses of a powerful therapeutic substance is violated when treating fungus infections with subeffective substances.

A different healing process is predicted by treating tinea pedis with an ideal fungicide. A spurious spread of the skin lesions may appear, resulting from the activation of quiescent foci of infection by toxins from killed fungi.

Trichophytid reactions should not constitute an obstacle to the use of powerful and strong true fungicidal substances.

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THE NEW YORK ACADEMY OF SCIENCES

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THE A. CRESSY MORRISON PRIZE CONTEST FOR 1950

The New York Academy of Sciences announces the following prize contest for the coming year:

Two prizes of $200 each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Meeting, December, 1950, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

CONDITIONS GOVERNING THIS CONTEST

(1) *Eligibility.* Authors and co-authors competing for prizes in this contest shall be members in good standing of The New York Academy of Sciences or one of its Affiliated Societies, but nonmembers may become eligible by joining one of these organizations before the closing date.

(2) *Date.* Papers are to be submitted on or prior to October 15, 1950, to the Executive Director of The New York Academy of Sciences, 2 East Sixty-third Street, New York 21, N. Y.

(3) *Papers.* All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached, together with an abstract suitable for press release.

(4) *Awards.* The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

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SECTION OF GEOLOGY AND MINERALOGY

COMPOSITION OF SOME NEW ENGLAND GRANITES[*]

By FELIX CHAYES[†]

The Present Status of Descriptive Petrography. The science of descriptive petrography has changed very little during the past sixty years. In the course of less than a generation it was brought from oblivion to a stage at which further progress was possible only along quantitative lines, and that is about where it is today.

For most of this development, we are indebted to the industry and skill of perhaps a half dozen men. The remarkable edifice they constructed still stands, and we still live in it. If our interest is in the qualitative aspects of description, then wherever we turn, we shall find we have been anticipated. We cannot hope to stand still indefinitely and must soon find answers to two questions. In the first place, could we, under any circumstances, develop a form of petrographic description related to conventional petrography in the way that quantitative analysis is related to qualitative analysis in chemistry? In the second place, should we do so?

These questions are by no means identical. An enormous amount of time and labor will be consumed in getting any kind of answer for the first. If the answer to the second is "No," all this time and labor will be lost. Thus, the second question is of paramount importance, and our answer to it must not be dictated by mere exhibitionism or a desire to outdo our grandfathers. Many things they did we do not have to do again. Jumping down out of trees was once a major undertaking for the whole race, or its precursors; today it is only a sport for small boys who are lucky enough to live where there are trees. In our day, a new demonstration of the organic origin of fossils would be an achievement of very modest dimensions. Unimpeachable quantitative evidence that granites contain less biotite than quartz would fall in the same category.

It is the author's opinion that a systematic quantitative petrography would be extraordinarily useful and that its development is entirely feasible. The present paper will concentrate on this question of feasibility and argue, first very generally and then by a specific example, that the availability of reliable quantitative modal analyses in sufficient number would have immediate and profound effect on petrological theory.

The Debate about the Origin of Granite. The modern science of petrology owes its birth to a bitter controversy over the origin of granite and, in one way or another, this question has always been before us. Since 1930, the discussion has become progressively less scientific and more personal and, last year, it began to go political. Magmatists, according to Perrin and Roubault,[1] are conservatives, while antimagmatists range through the remainder of the political spectrum to Reed, who is pink, and Perrin and Roubault, who are both redder than the rose. Of course, these two are not only extreme revolutionaries but devout liberals as well. The whole patter has a familiar, hollow ring. The present situation in genetics is a splendid example of its consequence.

Now, it is not hard to put the core of the granite dilemma in very simple terms. Must we suppose the existence and movement of large quantities of liquids (magmas) of granitic composition in the upper portions of the lithosphere at times and in places where there is little or no evidence that these liquids have pierced the crust and been discharged as extrusives?

The question is stated here as it might be put by a magmatist trying hard to be fair, for the magmatist is of course impressed by vast volumes of lavas of nongranitic composition whose existence and survival in the lithosphere *as liquids* no one seriously doubts. He is also inclined to regard the smaller, but still very respectable, volume of tuffs and extrusives of granitic composition as so much granite magma that has escaped from the crust. He thus feels no doubt that the key assumption is permissible, and the only question he will admit is whether it is necessary. If it is shown to be necessary, even the antimagmatist may ultimately give in.

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[*] This paper was presented at the meeting of the Section, February 6, 1950

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The antimagmatist, also trying to be fair, will probably insist—in fact, has insisted many times—that even this moderate statement of the issue reveals a latent magmatic bias. He is not prepared to discuss the necessity of the assumption until he is persuaded that it is permissible. If he can show that it is not permissible, the question of its necessity becomes academic.

Now, all this fanfare about protocol would pose a pretty problem indeed if geology were a branch of logic; it might be that we could never discuss the matter sensibly. In a natural situation as complex as this, however, the distinction between necessary and permissive hypotheses is far from sharp. What we really want is a useful hypothesis, or a hypothesis that is more useful than others. If it works, it is surely permissible; and no matter how well it works we can never be entirely sure it is necessary.

Concerning the composition of individual granite masses, or of groups of granites, classical petrography (petrography as we now practice it) never seems to generate the kind of evidence which would permit a firm and generally acceptable resolution of the controversy. If it can once swallow the curious fact that all granite masses are very similar in average or bulk composition, then, as far as composition is concerned, any hypothesis at all survives. In discussions of the origin of granite, the composition of granite thus plays at most a minor role. In fact, its very unimportance is sometimes cited as evidence of the strength of one or another hypothesis. Proponents of several of the transformist hypotheses openly boast that a granite can be made out of anything at all and offer no specification about the mineralogy of the finished product save that it is on the whole "granitic in composition."

I think we must regard this situation as one of the major failures of classical petrography, for the composition and variation in composition of a granite mass are among its most important properties. Indifference about major characteristics is never an indication of the strength of a theory. At best it reflects an appalling weakness in the data which the theory attempts to rationalize.

It is possible, though intuitively we can hardly consider it very probable, that hypotheses as different as crystallization from a liquid (with or without fractionation), crustal melting (with or without accumulation of the melt in a separate reservoir prior to recrystallization), solid diffusion, and the various forms of syntectic or migmatic processes would all yield bodies of the same average or bulk composition. With regard to internal variations in the composition of each mass, however, the assumption of an initially homogeneous parent material leads to inferences diametrically opposed to those drawn from other hypotheses. Reliable quantitative information about the internal variability of granite masses might thus go a long way toward settling the problem. For the magmatic hypotheses do in general contain the assumption of a homogeneous parent, while this assumption is conspicuously lacking in the nonmagmatic hypotheses, and its absence is usually intentional.

The Quartz Content of Some New England Granites. In order to keep the discussion within reasonable bounds, certain matters that ought to be treated very fully must be accepted on faith. In the first place, all the results used here are thin-section analyses made by point counter.[*] The analytical error is on the order of 1 per cent for major constituents and less than this for minor ones. Each analysis is of an area not less than ¾ nor more than 1 inch square. The analyses are all the author's own work. They perhaps reflect some personal bias in identification, but it is felt that this is at a minimum. The advantage of having all measurements by one man is that interoperator differences are eliminated. In the present state of the subject, these differences are a highly significant source of variation, in many cases, much more important than differences between rocks.

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[*] In this procedure, a symmetrical point grid is cast over the area whose composition is sought, material underlying each point is identified, and the total frequencies recorded are reduced to percentages. For a description of the technique see F. Chayes, Amer. Min **34**: 1–11, 1949.

Each thin section has been cut from a different hand specimen, and, with one exception, the hand specimens were never taken from sites less than fifty feet apart. Some are from different parts (usually different working faces) of the same quarry, but most are from different quarries. The quarries occur in irregular clusters in a roughly triangular area, bounded on the west by a line connecting Westerly, Rhode Island, and Woodbury, Vermont, and extending east to North Sullivan, Maine, on the north shore of Frenchmans Bay.* Nineteen quarry areas are represented in all, but from each of five areas fewer than five specimens were taken. In the others, the sample size ranged from seven to twenty-two, varying roughly with the number and size of quarries. It is with these that the discussion is chiefly concerned, though nothing in the smaller samples contradicts any of the findings. The sample is entirely confined to granites or granodiorites described by Dale[2] as other than coarse- or medium coarse-grained. It includes specimens from all the major commercial bodies of this type and is, I believe, by far the best sample of the finer-grained calc-alkaline granites of New England, so far subjected to quantitative modal analysis.

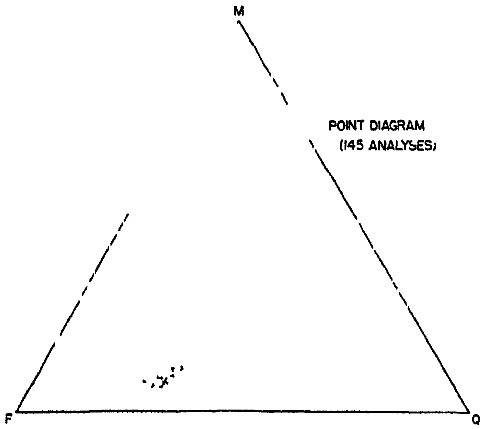

FIGURE 1. Point diagram of volume per cent of quartz (Q), total feldspar (F), and mafic minerals (M) in 145 thin sections of New England granite

Full details of sampling and analytical techniques, together with a complete table of analyses, will be ready for publication soon. For the present discussion, which is primarily intended to show that data of this type would be useful if we had them, the existence and application of adequate sampling and analytical techniques will have to be taken for granted. Finally, the place of a complete table of analyses will be taken by ternary diagrams of two types, both of which will be familiar to the reader.

The first series of projections is of the constituents Total Feldspar (F)—Quartz (Q)—mafic minerals (M), where M includes biotite, muscovite, opaque, and transparent accessories. The results for each granite are very similar and a summary of the total data is given in FIGURES 1 and 2. FIGURE 1 is a scatter diagram for the whole suite of 145 thin sections. Despite the close concentration shown on the point diagram, it is often a comparatively simple matter to distinguish these granites from each other on the basis of quartz content or color index. If space permitted an examination of the analyses instead of the diagrams, it would become evident that thin-section analysis is a technique far more powerful than is generally suspected.

For our immediate purpose, however, the remarkable similarity of all these results is much more important. FIGURE 2 is a contour diagram prepared from FIGURE 1 in the fashion common in petrofabrics except that a hexagon of 0.12 per cent area was used in place of a circle of 1 per cent. Notice that, even with this very small counter area, a 9 per cent maximum appears on the diagram. The 9 per cent contour encloses 0.5 per cent of the total area of the triangle and contains 47 per cent of the results. The total occupied area of the diagram is only 4 per cent. Concentrations of this order are unknown in petrofabrics; apparently even poles of mica cleavages in schist never approach it.

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* Westerly is a little over 200 miles south and North Sullivan a little over 200 miles east of Woodbury.

Now, if each point on the scatter diagram were in some sense an average value or an estimate of the bulk composition of a considerable mass of rock, we would be back just about where we started. The average composition of granite has been public knowledge for a matter of fifty years or so, and in some fashion every hypothesis manages to account for or ignore it. To some extent, too, the agreement about average or bulk composition is a matter of definition.

It is worth stressing again, therefore, that the points in FIGURE 1 are in no sense either averages or estimates of bulk composition. Each point is a plot of the result of a single analysis of a single thin section less than 1 inch in area. So far as it represents any real quantity of rock at all, the relevant volume is not that of a stock or a quarry or even a hand specimen; it is, at most, the volume of a single thin section, perhaps 1/1000 of a cubic inch. This absurdly small sample is taken in the worst possible fashion, with no sifting, rolling, or homogenizing of any sort. What is more, no two thin sections are from the same hand specimen, most of the hand specimens are from different quarries, and the quarries are widely scattered over New England. We are here in the presence of one of petrology's major mysteries, a mystery no serious hypothesis can afford to scoff at or fail to rationalize.

At this point, we may examine the assertion, made toward the close of the last section, that hypotheses based on the assumption of a homogeneous parent material for granite would give rise to inferences about internal variations very different from those generated by hypotheses from which this assumption was lacking.

If each granite was formed by undisturbed crystallization of a liquid of about the same composition as the finished product, it should occasion no surprise that each granite mass exhibits slight internal variation. With regard to color index, quartz, and total feldspar content, this proves to be the case. If the liquids differed in composition, we ought to be able to detect this difference, no matter how slight, by the use of large enough samples of each granite. The size of sample required for this purpose will vary with both intergranite differences and intragranite variation. Now, the existence of intergranite differences is easily detected by formal analysis of the data of FIGURE 1, and, as neither the sample size nor the total range of variation is large, we must conclude that the internal variability is very small indeed. It is, I believe, far smaller than the most sanguine magmatist would have anticipated. Evidently, we may not conclude either that there is a *single* granite magma, or that all granite magmas are of exactly the same composition. This complicates the matter, of course, but has nothing to do with the question of whether granite magmas exist.

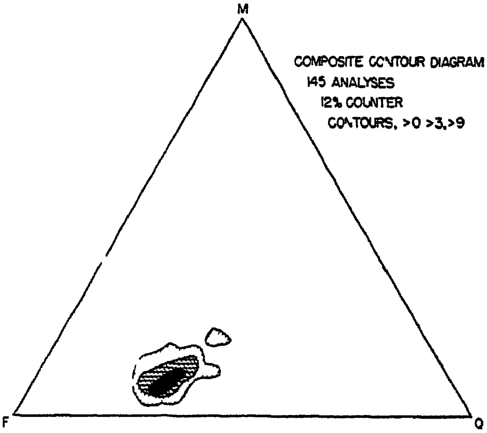

FIGURE 2. Contour diagram prepared from point diagram of FIGURE 1

According to nonmagmatic hypotheses,

the formation of granite is essentially a mixing process. A granitizing agent (ichor, emanations, clouds of ions) penetrates and reacts with a heterogeneous mass of sediments and metamorphic rocks. The pre-existing rocks take just what they need from the invading medium and surrender to it exactly what they do not need. The end product is granite. It is practically impossible to discuss a process of this type in terms that are both general and meaningful, but we can come to grips with it if we are willing to consider variations exhibited by some constituent abundant both in the pre-existing rocks and in the end product.

Sandstone and quartzite, for instance, are a common part of any sedimentary or metamorphic complex. Schuchert[3] estimates that they form about 25 per cent of the sediments of the Appalachian trough, and we may use this as a first approximation for New England. (It makes little difference whether the correct value is 20 or 30 per cent.) From the work of Tallman,[4] we would anticipate that nearly half of this material carried over 95 per cent of quartz and that in about two thirds of it the quartz content was over 75 per cent. Of a little over 200 analyzed thin sections of New England granites in my collection, not one carries as much as 40 per cent of quartz, and in only three is the quartz content over 36 per cent. What has become of the sandstones and quartzites? Evidently they are gone without trace. Would we not anticipate that a feldspathization process so extreme would overshoot a little, forming, here and there, minute patches (of the order, say, of a few square inches or parts of a square foot) of syenite? It does not seem to, and any such patches which do occur must be extraordinarily rare. I have not so far found a single thin section of New England granite containing less than 20 per cent quartz. What kind of feldspathization process is it that works perfectly wherever it finds more than 40 per cent of quartz and fails completely when it reduces the quartz content to 20 per cent? Such a process operating on a very coarse scale and speaking only in terms of average or bulk compositions is hard enough to credit. What are we to say when it works at a scale of square inches of area and on volumes of the order of a thousandth of a cubic inch?

Unreasonable as all this seems, something of the sort must be involved in each of the nonmagmatic granitizations of highly siliceous sedimentary or metamorphic rocks. To the extent that this is so, I believe we must discard them outright as explanations of the origin of these particular granites. A similar argument could be made out for other minerals, particularly biotite, muscovite, and total feldspar, but it would necessarily be more tedious and less persuasive, since reliable quantitative information about the modal composition of metamorphic rocks is virtually lacking.

Relations between Quartz, Orthoclase, and Plagioclase. We have so far found strong reason for preferring hypotheses which presuppose the existence of a homogeneous parent material for each granite mass. The implicit assumption that this homogeneous parent must be magma is just the type of gratuitous extrapolation to which the antimagmatists object so strenuously; and their objection is often quite proper. The extrapolation is always dictated by convenience, but in the present case there is much more to it than that. The reason for selecting a liquid as the homogeneous parent material is that in each granite so far examined, the relations between quartz, orthoclase, and plagioclase are just what would be expected in samples of a crystallizing liquid. In most of these cases, the relations actually developed would not be anticipated in samples of anything else.

In FIGURES 3 to 6, which show the proportions of quartz, orthoclase, and plagioclase found in thin sections of individual granite masses, a slanted rectangular field extends upwards from near the center of the base line to a little past the center of the triangle. This is the silica-rich end of the thermal valley in the system, nepheline-kaliophilite-silica. Its significance has been admirably described by Dr. Bowen,[5] who showed that: (a) fractional crystallization of a liquid of basaltic composition would generate residual liquids lying in or near the thermal valley; and (b) in the vast majority of cases, compositions of individual glassy rocks or averages for rock glasses of different types do in fact lie in or very near the thermal valley.

Our argument has nothing to do with glassy rocks, average values, or fractional crystallization of basaltic liquid, yet the distribution of values for each individual granite mass shows a curiously systematic relation to the thermal valley.

For certain granites, as shown in FIGURE 3, *all* results lie in the valley, a phenomenon perhaps not so critical for eucrystalline rocks as for glasses. There are three examples of this type in my sample: Westerly, Rhode Island; Bradford, Rhode Island; and Milford, New Hampshire. In each of the other nine New England granites for which I have suitable data, some points lie outside the valley, but in only one (Westwood, Massachusetts) are they all outside. Thus, in eight of the nine cases there is some overlap of the valley boundaries, and the nature of this overlap is highly critical, for *in any particular granite, values lying outside the valley are all on the same side of the valley.**

In the granites of North Jay-Hallowell, Maine, Westwood, Massachusetts, and Pownal, Maine, outlying values are all on the orthoclase side of the valley (FIGURE 4 shows the data for Pownal). In the granites of Fitzwilliam and Concord, New Hampshire, and Clark Island, Maine, all overlap is in the quartz field (the data for Clark Island are shown in FIGURE 5). In the granites of North Sullivan, Maine, and Barre and Woodbury, Vermont, overlap is exclusively on the plagioclase border of the valley (FIGURE 6 shows the data for Woodbury).

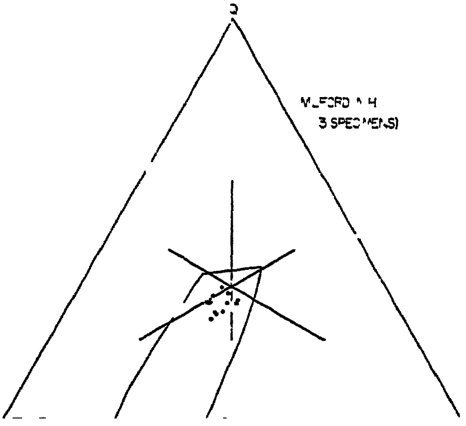

FIGURE 3. Proportions (by volume) of quartz (Q), plagioclase (Pl), and orthoclase (Or) in 13 thin sections of Milford, N. H. granite. All values in thermal valley.

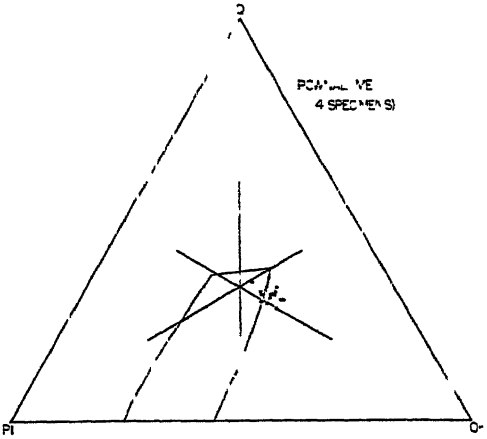

FIGURE 4 Fourteen thin sections of Pownal, Me., granite, showing overlap of thermal valley on orthoclase side. Coordinates as in FIGURE 3.

In every one of these nine cases, a reasonable shift of the ternary mean would have generated overlap on two sides of the valley. In three of the nine, dispersion is easily large enough that a rather small shift of the ternary mean, depending on its sense, would have generated either a two- or threefold overlap. Yet the single overlap is the only type so far observed.

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* A few minor exceptions should perhaps be mentioned. Of a total of twenty-two thin sections of Barre, results for two lie not only well to the left of the plagioclase margin of the valley, but slightly above the boundary of the quartz field This is true also for one of the eight slides of Woodbury and two of the ten slides of North Sullivan. In all four cases, the discrepancy is considerably smaller than analytical error

It is to be noted, also, that the valley is outlined for the pure system Ab-Or-Q, while the modal values are for plagioclase containing some anorthite and orthoclase probably containing a little albite. It is hard to see how any such systematic relationship could develop unless, on the whole, the amount of Ab in Or was of the same order as the An content of plagioclase. The plagioclase of all these granites is normally intermediate oligoclase, albite and andesine both being extremely uncommon. The Ab content of modal orthoclase cannot be satisfactorily determined without extraction of the mineral from each rock. This work is now in progress.

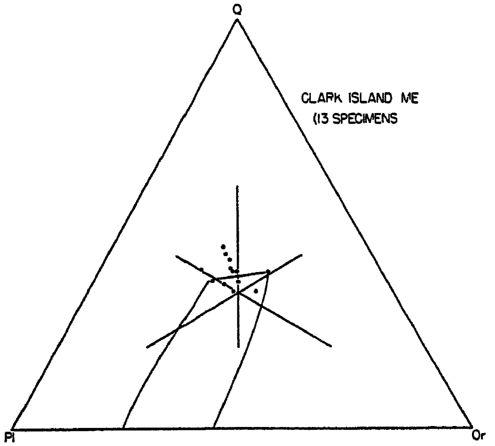

FIGURE 5 Thirteen thin sections of Clark Island, Me., granite, showing overlap of thermal valley on quartz side Co-ordinates as in FIGURE 3

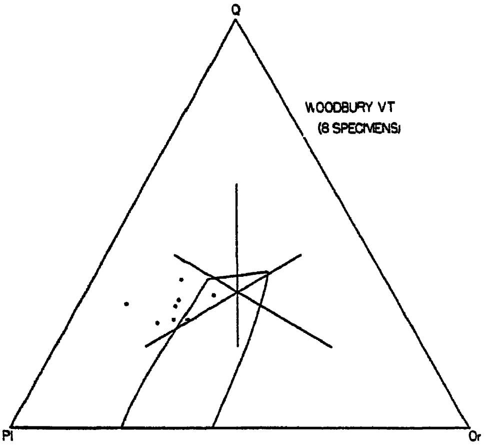

FIGURE 6 Eight thin sections of Woodbury, Vt., granite, showing overlap of thermal valley on plagioclase side. Co-ordinates as in FIGURE 3

By any of the nonmagmatic hypotheses this effect must be regarded as a chance event. As far as these hypotheses are concerned, points might fall outside the valley on any one side, on any two sides, or on all three sides. Of these seven possibilities, only three are realized, and all are of one type; *viz.*, the single overlap. Because of the variability of dispersion, no exact probability can be attached to this event. A rather large scatter is required for a triple overlap, while the width of the valley clearly sets a maximum value for cases of no overlap. If we suppose, however, that single and double overlaps are equally likely, the probability of obtaining nine of one type and none of the other is only $(\frac{1}{2})^9$ or .002 If we suppose that single overlap is twice as likely as double, which is surely an overestimate, this probability is still only $(\frac{2}{3})^9$, or .026. The correct answer is probably somewhere between these limits. By standards usually applied in this type of work, the probability of the observed frequency of single overlaps is so small that its occurrence as a chance event is extremely unlikely. Hypotheses which can only regard it as a chance event (and this includes, I believe, *all* nonmagmatic hypotheses) are by the same token rendered extremely unlikely.

For magmatic hypotheses, the situation is entirely different. The observed distribution of Q-Or-Pl values is reasonable and almost necessary. We could tolerate occasional instances of double or triple overlap, of course, but if such cases proved numerous we would have to conclude either that the experimental technique was faulty or that the theory was wrong. Crystallization of a liquid whose initial composition lies in the diagram generates a liquid residue which approaches and finally enters the thermal valley, and, unless heat is added, the liquid cannot escape from the valley once it has entered. The composition of adequate samples of such a crystallizing mass would necessarily fall either in the valley or outside it on the side on which the initial composition lay.

Summary and Conclusions. The magmatic hypothesis (by which I mean simply the notion that each of these granite masses has formed by crystallization of a liquid of approximately the same composition as the finished product) thus survives two rather critical tests, both of which throw strong doubt on the whole suite of nonmagmatic explanations. With regard to internal variations in composition (and quartz variation is only one of several possible examples), the nonmagmatic hypotheses would seem to require dispersion decidedly greater than that actually found. With regard to variations in the relative proportions of quartz, plagioclase, and orthoclase in each mass, they offer no reason for expecting any particular type of distribution and are unable to regard as more than a chance event the peculiarly systematic type of distribution which does actually emerge.

Now, it is not all smooth sailing for the magmatic hypotheses either. The conclusion that there is no *single* granite magma, first reached from the Q-M-F projections, is strongly affirmed by the Q-Pl-Or distributions. There are at least four distinguishable types of parent material, and it is not unreasonable to suppose that no two granite magmas are identical, even with regard to major constituents. Further, the whole discussion of Q-Pl-Or relations is something of a *tour-de-force.* The thermal valley is developed for the pure system Q-Or-Ab; in real granites there is invariably some anorthite in plagioclase and some albite in orthoclase. Although they complicate the discussion considerably, uncertainties introduced by this difficulty are probably rather small.

Finally, nothing has been said about the origin of magma, and in fact the evidence throws no new light on that problem. It is often argued that the magmatic origin of granites is suspect, because no known process could generate enough magma. But a pedestrian does not refuse to step out of the path of an approaching truck because he cannot be sure it was made in Detroit. I do not know how or where granite magmas get themselves manufactured, how they enter or move about in the crust, or how they manage to maintain their identity prior to crystallization. If, however, we seek a rational explanation of the variations in composition exhibited by the finer-grained calc-alkaline granites of New England, it seems to me we shall have to suppose that, in some fashion, they manage to do all three of these things.

The validity of this conclusion hinges, of course, on the adequacy of the sampling procedure, and the need for adequate sampling techniques in petrography is little short of desperate. The conclusion will no doubt be extremely unpopular in some quarters. It is useful to point out that merely questioning the sampling procedure is not enough. Anyone who dislikes the conclusion enough to want to do something about it will have to do a great deal. Briefly, he will have to develop a different sampling technique and obtain for samples collected in the new fashion a number of quantitative modal analyses at least comparable to that used here. Information accumulated in this fashion would be of infinitely greater value to the science than the groundless speculative quarrels which now occupy us. In the end, we would at least have something to talk about.

However this may be, I believe sufficient evidence has now been offered in support of the assertion that the availability of large numbers of reliable quantitative modal analyses would immediately and powerfully affect discussions of the origin of rocks. A systematic quantitative petrography would be well worth having.

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SECTION OF PHYSICS AND CHEMISTRY

THE SEARCH FOR LONG RANGE FORCES IN SOLUTION[*]

By BENJAMIN CARROLL[†]

In a series of publications,[1] Rothen has presented experimental evidence which strongly suggests that antigens and homologous antisera can react in the presence of an intervening film. The film, usually referred to as a blanket, may be several hundred Ångstroms thick. This work has been extended to include enzymes. Here again it appears that direct contact between enzyme and substrate is not essential for enzymatic activity.

The Rothen technique is direct in its approach to this problem. A slide is covered with one or more monolayers of protein antigen. A film of known thickness of inert material, such as Formvar, polyvinyl chloride, or barium stearate, is deposited on top of the antigen film. Finally, a drop of dilute solution of the corresponding immune serum is spread over the blanket. After the slide is washed, specific adsorption of antibody can be detected. In the case of enzymes, a drop of enzyme solution is used in place of the immune serum. The effect of the enzyme acting through the blanket can be determined by testing subsequently for the specificity of the antigen film. Loss of specificity for homologous antisera indicates that enzymatic action has taken place across the inert blanket.

To account for this phenomenon, Rothen has postulated the existence of long range forces which can act over a distance of several hundred Ångströms.

Contrary to this viewpoint is the evidence that has accumulated from the work of many immunologists.[2] Their data indicate that serological specificity is dependent to a large degree upon "lock and key" complementariness as suggested originally by Paul Erlich. Pauling[3] and coworkers have shown that a portion of the antibody molecule fits the antigen group so well that immunochemical techniques may be used to determine the configuration of molecules and haptenic groups. They have shown that the forces involved in the specific combination of antibody and antigen require approximation of the attracting molecules to within one or two Ångströms.

To help resolve these conflicting viewpoints, we have looked for the Rothen effect in solution. Conditions similar to the Rothen experiment on enzymatic action were used. The substrate, bovine serum albumin, and the proteolytic enzyme, pepsin, were selected. The plan was to coat the albumin molecule with an inert layer or blanket by the adsorption of ions or molecules and then investigate the effect of the enzyme upon the coated substrate.

According to Rothen, long range forces should be exhibited if the bulk conductivity of the blanket is practically zero. In selecting substances to coat the albumin in solution the choice was limited more or less to paraffin chain salts or dyes. The use of paraffin chain salts was open to several objections. Their effect on proteins has been reported to be harsh, frequently resulting in the denaturation of the protein.[4] A more serious objection is the uncertainty in estimating the area on the substrate molecule that becomes inaccessible to the enzyme when a known number of paraffin chain molecules are adsorbed. Whether the paraffin chains would be closely adlineated, with their tails pointing toward the liquid phase, or randomly kinked or lying flat on the surface of the protein is unknown. In building up a blanket with dye molecules, the uncertainty in the estimated area of substrate blocked by a dye molecule is considerably less.

The number of dye molecules required to form a unimolecular blanket around the bovine albumin molecule, which has a molecular weight of 69,000, will depend upon the model taken for the protein as well as the dye. If the albumin molecule is assumed to be spherical, its surface area is about 10,000Å^2. Viscosity, dielectric dispersion, and diffusion data point to an asymmetric molecule.[5] These measurements are consistent with a model of a prolate ellipsoid having an axial ratio of 1:5. This would yield a surface area of some 25,000Å^2 for the albumin molecule.

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[*] This paper, illustrated with lantern slides, was presented at the meeting of the Section, February 7, 1950

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Azo dyes having a molecular weight of about 350 were used for the blanket. The area of protein that would be covered by a dye molecule is 60Å^2 if the dye is considered cube-shaped and about 150Å^2 if it is spread flat on the surface of the protein. A convenient approximation of 100Å^2 taken as the average of these two extreme configurations indicates that 200 to 300 dye molecules spread evenly over the surface of a bovine serum albumin molecule may be considered a unimolecular blanket having a thickness of a few Ångströms.

At first, attempts were made to build up a blanket with so-called fat-soluble dyes.[6] Such a blanket was sought because the probability of having the enzyme rapidly displace the dye molecules and thus gain access to the substrate seemed remote. These attempts were abandoned because an insufficient number of dye molecules were adsorbed by the protein.

Water-soluble acid dyes which follow Beer's law were then used. Since the number of sites available to these dyes on the alkaline side of the isoelectric point of albumin[7] is about $\frac{1}{10}$ that required for a unimolecular layer, adsorption studies were undertaken in 0.01M HCl. The total concentration of dye was adjusted to a value that gave a reading in the sensitive range of the spectrophotometer, since this instrument was used to determine the amount of dye adsorbed by the protein. The extinction for 2.0 × 10^{-5} M Orange I in the presence of bovine albumin is shown in FIGURE 1.

The results are similar to those given by Klotz.[8] There are two points of difference however. One is that the extent of the adsorption is increased by several orders of magnitude. Secondly, the adsorption is distinctly a two-step process, where at first 100 to 130 dye molecules are adsorbed by a single albumin molecule. This occurs immediately upon mixing the dye and protein. In addition, there is a more deliberate uptake of dye over a period of several weeks, so that an apparent upper limit of some 250 dye molecules per protein molecule is attained. These results are indicated in FIGURE 2. It will be seen that in order to maintain this upper limit the molecular ratio of dye to albumin in the solution must be about 10^6. This means that, when one part per million of albumin is present in 2.0 × 10^{-5}M Orange I, a sufficient number of dye molecules is adsorbed to form a unimolecular blanket.

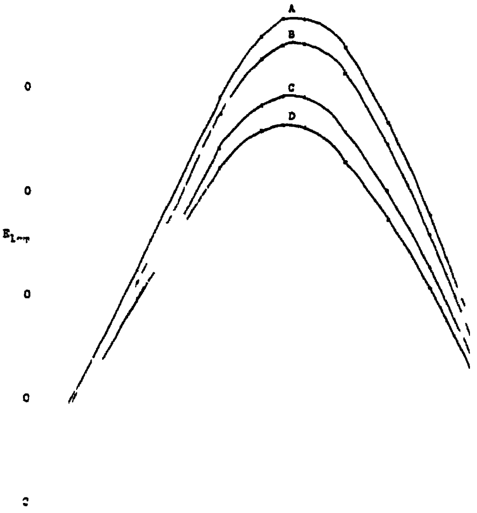

FIGURE 1. The extinction of 2.0 × 10^{-5}M Orange I in presence of crystallized bovine albumin and 0.01M HCl. Readings taken 10 minutes after mixing. Temperature 27°C. Concentration of bovine albumin in p.p.m.: A—0.0, B—2.0, C—10.0; D—100.0.

It was thought that the slow uptake of dye was due to the progressive denaturation of the albumin in 0.01M HCl. Experiments[9] indicated that bovine albumin was remarkably stable in acid solution and that the coated albumin could be regenerated by dialysis. The binding capacity of the albumin for simple dye anions at pH 7 was used as a criterion of denaturation.[10]

A question to consider is the disposition of the dye molecules on the coated albumin. The fact that the bovine albumin molecule has 112 cationic groups would indicate that the initial adsorption of dye takes place at these sites.[11] The subsequent uptake of additional dye may be occurring either around these sites or, most likely, on the bare surface of the protein. In any case, the adsorbed dye may be viewed more or less as an evenly spread blanket.

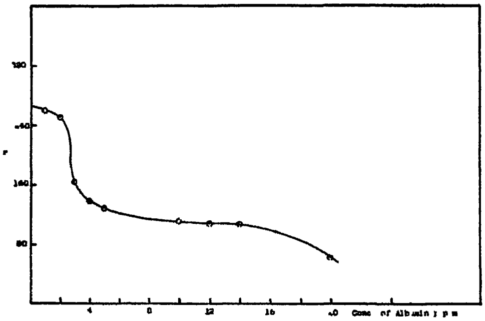

FIGURE 2 This graph shows (r) the number of dye molecules adsorbed per protein molecule after allowing 2 0 × 10^{-4}M Orange I to remain in contact with bovine albumin for 30 days at 0°C Solutions were equilibrated for 6 hours at 27°C before spectrophotometric readings were taken to determine the extent of adsorption

The effect of a proteolytic enzyme on a substrate concentration as low as one part per million required the development of a new method of determining the activity of proteolytic enzymes.[12] The method had to detect any possible denaturing effect as well as hydrolysis of the substrate, since in the Rothen experiment either one or both effects may be responsible for the loss of specificity of an antigen.

It was observed that proteolytic enzymes caused the bovine albumin molecule to lose its combining capacity for simple dye anions. Spectral curves similar to those shown in FIGURE 1 were obtained for an albumin-pepsin solution when Orange I was present. As the reaction proceeded, the absorption curve approached, and in some instances reached, the spectral values for the dye alone. Since the products of hydrolysis of bovine albumin as well as of the enzyme had very little affinity for Orange I in 0.01M HCl, the changing spectral curves could be interpreted readily in terms of changing concentration of the native albumin. Such factors as temperature and initial concentration of substrate or enzyme were investigated, and the usual kinetic formulas could be applied.

The spectral method for detecting enzymatic activity was well suited to systems having initial concentration of substrate as small as one part per million. In addition, the method could detect the effects of one part per billion of pepsin. When a concentration of enzyme as large as 0.003 per cent was used, the half-life of the native albumin-pepsin reaction was always less than ten minutes and frequently less than one minute, the time depending upon the initial concentration of substrate.

To measure the velocity of the reaction, two procedures could be used. The dye could be present during the reaction or added to an aliquot of the dye-free reaction mixture at various time intervals. If 'r,' the molecular ratio of adsorbed dye to albumin, was small, both procedures gave the same results. As 'r' increased beyond the value of 100, the presence of the dye slowed down the reaction appreciably but did not stop it. This will be seen in FIGURE 3. The results in FIGURE 3 suggest that the loci of attachment of the dye at values of 'r' greater than 100 may be near the peptide linkages, because of the pronounced effect of the dye in inhibiting the reaction. As the value of 'r' increases, the reaction is slowed down to a point where, approximately, a unimolecular blanket inhibits the albumin-pepsin reaction completely. Similar results were obtained when the pepsin concentration was increased to 0.1 per cent. It is interesting to note that at 'r' = 114, where the albumin has its particle weight increased 50 per cent, the blanket offers little protection. Some may interpret this as a Rothen effect. Nevertheless, the fact that a blanket which can be considered unimolecular in thickness completely inhibits the reaction between albumin and pepsin is strong evidence in favor of the viewpoint that intimate contact between substrate and enzyme is essential for enzymatic activity.

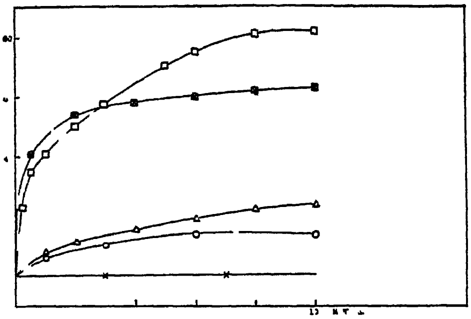

FIGURE 3 The effect of 003 per cent crystallized pepsin on albumin in presence of 2 0 × 10^{-5}M Orange I Temperature 27° ± 0 1°C Concentration of HCl is 0 01M

Initial conc. of albumin	*p p m*	*Initial ratio of adsorbed dye to albumin* 'r'
××××	1	200
○○○○	3	162
△△△△	4	142
⊠⊠⊠⊠	10	114
□□□□	1000	1 4

The literature contains one type of experiment on the kinetics in solution of a protein-enzyme system which has been offered as possible evidence for the existence of the Rothen effect. It is the experiment of Northrup,[13] showing that the digestion of casein and of gelatin proceed at the same rate whether present in separate solutions or in the same solution. The enzyme used was trypsin. Apparently, the concentration of enzyme to substrate was sufficiently small to yield a reaction velocity of zero order.

The spectral method for enzymatic activity can measure the effect of enzyme on albumin in the presence of other substrates which do not adsorb the dye indicator. Orange I at 2.0 × 10^{-5}M is not adsorbed to any appreciable extent by gelatin in 0.01N HCl. The effect of gelatin on the rate of the albumin-pepsin reaction is shown in FIGURE 4. Initially, there is ten times as much gelatin as there is albumin, yet the velocity of the reaction is not appreciably affected. The reaction which is zero order has a half-life of 15 minutes. The final points on the curve represent the reaction at 90 per cent completion.

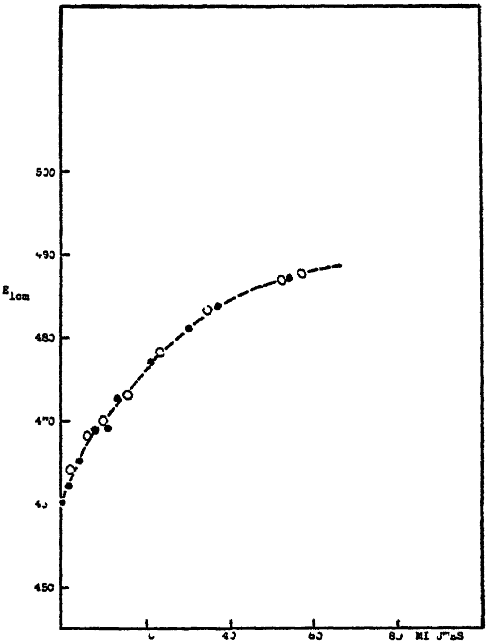

FIGURE 4 Effect of gelatin on reaction rate of crystallized bovine albumin (016 per cent) and crystallized pepsin (4 0 × 10^{-4} per cent) in 0 01N HCl Orange I (2 0 × 10^{-5}M) present during reaction, temperature, 30°C The extinction of the reaction mixture at 4750Å is plotted as a function of time ● ● ● ●—reaction of albumin and pepsin in absence of gelatin ○ ○ ○ ○—reaction of albumin and pepsin in presence of 0 16 per cent gelatin

The results of this experiment are in complete accord with those of Northrup. The author is reluctant, however, to accept the postulate of long range forces as an explanation. Michaelis[14] has suggested that proteolytic enzymes may have enzymic sites with different specificity.

The implications of the Rothen effect in the natural sciences are obviously enormous. Unless the effect is demonstrated in the liquid and solid state, it is doubtful if the scientist will accept it. The reason for this is the practical and theoretical experience that has accumulated for these two states of matter. By comparison, surface chemistry, particularly the behavior of films on solid surfaces, is a very recent field. Electron microscopy and electron diffraction are basic techniques in this subject but are relatively new and still inaccessible to most investigators. Undoubtedly, the Rothen effect will do much to stimulate research in surface chemistry.

It may be considered that special orientation effects in films normally absent in solution would be responsible for the Rothen effect. Some of the strongest evidence, however, for the existence of the intermediate complex in reactions and that contact between reactants is essential can be gathered from the kinetic studies of unimolecular films on liquids.[15]

In solution, hapten inhibition of antigen-antibody precipitation or the inhibition of protein-enzyme reactions by a unimolecular blanket are strong arguments against the existence of the Rothen effect. Perhaps the crucial test for the Rothen effect and long range forces would be the finding of appreciable enzymatic activity in the solid state.*

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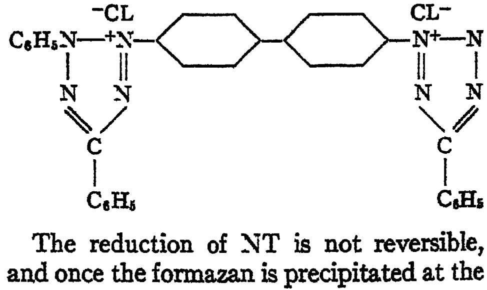
* The author is indebted to Prof. A. W. Thomas for his encouragement and helpful suggestions regarding this work.

SECTION OF BIOLOGY

THE USE OF NEOTETRAZOLIUM AS A TOOL IN THE STUDY OF ACTIVE CELL PROCESSES*

By WILLIAM ANTOPOL, SUSI GLAUBACH, AND LESTER GOLDMAN†

The selective staining of tissue by pp′ diphenylene bis 2-(3,5 diphenyl tetrazolium chloride), *in vivo* and *in vitro*, was described in a previous report. The substance is called neotetrazolium and will hereafter be referred to as NT. The conversion of the pale yellow compound to an insoluble deep purple to black compound is due to the reduction of NT, forming a colored formazan which takes place at the site of active metabolic processes.[1] The formula for NT is



The reduction of NT is not reversible, and once the formazan is precipitated at the site of reduction in the living cell, it remains *in situ* for some time. It was therefore of interest to study the influence of chronic administration of NT in animals.

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* This paper, illustrated with lantern slides, was the first of three papers presented at the meeting of the Section, February 13, 1950.

† The Joseph and Helen Yeamans Levy Foundation, Beth Israel Hospital, N. Y. and the Laboratories of the Newark Beth Israel Hospital, Newark, N. J.

In this communication, we will report on the appearance of organs after intraperitoneal, subcutaneous, and oral administration of NT to white mice, the influence of intradermal injection of NT on the hair color of C_3H (brown), *dba* (gray), and C_{57} (black) mice and the effect of subcutaneous injection of NT on the growth of immature rats. In all the experiments, NT in a concentration of 0.1 per cent is used.

Intraperitoneal Administration of NT to White Mice. Sixty-nine male and female mice, weighing 25 to 30 grams, received daily injections of 0.5 cc. NT for periods varying from 1 to 60 days. The animals were sacrificed at varying intervals. In one hour, there was a slight pink tint with a purple hue to the peritoneum, and the omental and peritoneal fat stained faintly pink. At this time, there was a slight but distinct purple color in the liver and the cortex of the kidney. This was more pronounced after 24 hours. After 48 hours, the liver, kidney cortex, and outer areas of the medulla became deep purple. The mucosa of the stomach in the region of the gastric glands was faintly purple, while the nonglandular portion, the cardiac antrum, and the region containing the pyloric glands remained unaffected. In 3 days, the liver was deep purple to black. The kidney cortex also assumed this color and a faint color appeared in the medulla. The sublingual and submaxillary salivary glands and the mammary glands were purple; in the parotid, the color change was not as distinct. The intermediate lobe and, to a lesser degree, the anterior lobe of the pituitary, the interscapular fat pads, the bone marrow, and the prostate took on a purple tint. The reticularis zone of the adrenal cortex was distinctly purple. The ovaries, particularly the corpora lutea, also assumed this color. As time went on, the changes were more pronounced in all of these organs. The renal medulla became progressively darker, so that it approached the black appearance of the cortex. The entire animal took on a purple hue and most organs, including the lymph nodes, thymus, and muscle and lacrimal glands, were stained. The brain, testes, and seminal vesicles retained their natural color. The dorsal ganglia became darkly stained. The sympathetic ganglia were not seen.* Because of the normal purple color of the spleen, it was difficult to determine the degree of gross change in this organ.

When mice were treated on 4 successive days, after which treatment was discontinued, the staining of the abdominal organs was still intense 2 weeks after the last injection, becoming less distinct after 4 weeks. The kidney was pale: but a light purple color remained in the liver and the remaining intra-abdominal fat was still dark purple. When the mice were dosed continuously for 2 to 3 weeks, distinct staining of the organs could still be observed 3 months after the last injection.

Microscopic. Histologic examination of the tissues in which the color developed showed accumulation of formazan as fine black granules, at first in the perinuclear zone and, as time went on, throughout the cytoplasm. In the kidney, the NT reduction took place initially in a linear arrangement near the free end of the convoluted tubules. The localization and arrangement of the granules suggested that reduction of NT occurred in organoid structures of the cell cytoplasm. The color change occurred in the cytoplasm and was never observed in the nuclei of normal cells. Detailed histologic studies of each organ will be the subject of a future communication.

Subcutaneous Administration of NT to White Mice. In one series, twenty-five mice were injected subcutaneously with 0.5 cc. NT daily for from 1 to 5 days. In a second series, twenty-five mice received injections every other day for 2 months. Coloring of the organs took place as with the intraperitoneal administration, although at a slower rate. The intra-abdominal fat, however, remained unstained. If treatment was continued for longer than 3 weeks, the intra-abdominal fat diminished in amount or disappeared.

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* The effect on the nervous system is being investigated in collaboration with Drs. Abner Wolf and David Cowen, College of Physicians and Surgeons, Columbia University, New York City.

Oral Administration of NT to White Mice. Twenty white mice fed daily with 0.5 cc. to 1.0 cc. NT from 8 to 60 days showed color changes very irregularly which were considerably less pronounced than with subcutaneous injection. The liver and kidney changes were less irregular.

Administration of NT to Colored Mice. Twenty-two male and female brown mice (strain C_3H) were injected subcutaneously with 0.5 cc. NT every other day for 1 month and then mated. None of the females became pregnant, whether mated with NT-treated males or with untreated males. The appearance of the organs was similar to those in white mice. The ovaries were very small. The mammary glands also stained purple. Similar results were obtained in *dba* gray and C_{57} black mice. In a series of C_3H mice with spontaneous mammary tumors, the neoplasm stained intensely. Observations on the effect of NT on the occurrence of mammary tumors in this strain is under investigation, as well as on the effect of NT on the growth of mammary tumor *per se.**

In the experiments with chronic subcutaneous administration of NT in C_3H mice, it was observed that in some mice bald areas and thickening of the skin appeared at the site of frequent NT injection. When hair grew back, it was not its natural brown color, but white. This phenomenon did not occur in the intraperitoneally treated mice. The question arose whether the appearance of white hair was not due to accidental intradermal injection of NT. To see the effect of intradermal injection of NT, the backs of male and female C_3H mice were partly shaved, and the next day the mice received a single intradermal injection of 0.5 cc. NT; the control mice received 0.5 cc. of NaCl (0.85 per cent). The hair in the control animals grew back in its natural color. In NT-injected mice it grew back white, and the white hair was only to be seen in the injected area. This experiment was repeated in *dba* gray and C_{57} black mice with the same results. The hair remained white until the mice died, the longest period of observation being 5 months. In one of the mice, the white hair was plucked out; the hair grew back white. It appears that NT injected intradermally, when reduced to its insoluble formazan might inactivate or interfere with the activity of enzyme systems concerned with pigmentation. Investigations in this direction are under study.

Subcutaneous Administration of NT to Immature White Rats. Fifteen young rats weighing 35 grams received subcutaneous injections of NT every other day starting with 0.3 cc. and increasing the dose to 0.5 cc. and then to 1.0 cc. TABLE 1 demonstrates a typical example of the growth-inhibiting effect of NT in these animals compared with control rats. It is worthy of note that the fur remains dull and immature in NT-treated animals even after 5 months. The color changes in the organs were similar to those observed in mice.

TABLE 1

	Control	*NT treated*	
Date	*body weight*	*body weight*	*cc. of 0.1 % NT every other day*
7–9–48	36 gm.	40 gm.	0.3
7–23–48	86 gm.	58 gm.	0.3
8–4–48	145 gm.	90 gm.	0.3
8–16–48	184 gm.	132 gm.	0.3
8–23–48	218 gm.	156 gm.	0.3
9–2–48	229 gm.	178 gm.	0.5
9–14–48	254 gm.	206 gm.	0.5
9–25–48	290 gm.	230 gm.	1.0
10–2–48	307 gm.	246 gm.	1.0
10–15–48	328 gm.	269 gm.	1.0
10–28–48	338 gm.	285 gm.	1.0
11–19–48	378 gm.	297 gm.	1.0

Discussion. The ability of NT to act as an hydrogen acceptor in the metabolic processes of living cells and to be converted to the insoluble formazan at the site of reduction[1-3] makes it valuable not only as a tracer for intracellular localization of metabolic processes, but also as tool for determining whether certain drugs have a depressing effect on the activity of dehydrogenases. It is known that trivalent organic arsenical derivatives have a great affinity for —SH groups.[4] Since many dehydrogenases have free —SH groups which determine the activity of these enzymes, these derivatives have been used as inhibitors of dehydrogenases. We have shown that pretreatment of tissue *in vitro* with neoarsphenamine abolishes the ability of the tissue to reduce NT.[1] NT can also be used to demonstrate selective destruction or damage of certain structures in an organ (such as occurs in the kidney after administration of uranium salts to rats). Such a damaged kidney shows diminished ability to reduce NT *in vivo* and *in vitro*. This will be reported in detail in a subsequent communication.

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* The mammary tumor effects are under investigation in collaboration with Dr. Samuel Graff, College of Physicians and Surgeons, Columbia University, New York City.

NT is not only a tracer of hydrogen donors, it can also be an inhibitor of dehydrogenases. It has been observed that an *in vivo* injection of 0.5 cc. of a 0.1 per cent solution of NT into a rat muscle which is excised 24 hours later prevented this muscle from decolorizing methylene blue by the Thunberg method, in contrast to the corresponding untreated muscle of the opposite side.[1] These findings indicate that the process of reduction of NT and precipitation of formazan at the site of injection interferes with metabolic processes, *i.e.*, dehydrogenases. The findings herein reported (namely, the retarded growth and dullness of hair of immature rats treated subcutaneously with NT, the disappearance of intra-abdominal fat in NT-treated mice and rats, and the growth of white hair after a single intradermal injection of NT in gray, brown, and black mice) point again to disturbances in metabolic processes.

Of interest is the fact that spontaneous breast tumors reduce NT both *in vitro* and *in vivo*, while sarcoma 180 reduces NT very slightly *in vivo* but intensely *in vitro*. This poses the question whether NT lodges and is reduced in tumors arising from cells in organs which normally reduce NT *in vivo*. Investigations are in process to determine the effect of various tetrazolium and related compounds on tumors, particularly those which arise spontaneously in organs in which the tetrazolium compounds are reduced. The effect of compounds containing radioactive isotopes on these tumors will also be studied.*

It is also noteworthy that certain organs, such as brain and testes, and some tumors, such as sarcoma 180 and Flexner Jobling tumors, stain supravitally but not intravitally when injected with NT at a site distant from the tumor. The question of the existence of blood-tumor and blood-testis barriers should be considered as one does that of the blood-brain barrier.

The effects of NT and related compounds on viruses are of interest. We have already shown that certain organisms thrive in the presence of NT while others are retarded and still others are killed by it. The fact that certain viruses have active —SH groups and others do not indicates that the effects may very well differ with viruses as they do with bacteria.[1] Since certain tumors are caused by viruses, such as the milk-factor tumor of mice, the effect on the tumor as well as on the virus *per se* is to be studied.*

In studying the gross color changes, widespread as they are in chronically NT-treated animals, it becomes necessary to distinguish microscopically between the reduction of NT which takes place *in situ* and the mobilization and subsequent storage of colored compounds which are taken up by macrophages, and, to a lesser extent, endothelial cells, particularly in the liver and the adventitial zones of blood vessels. The different shades of color which can be seen in the organs raises the question whether they are due only to a variation of concentration of reduced NT, to different compounds, or to compounds in combination with cell constituents. Extraction experiments with different organic solvents indicate that concentration alone cannot account for the various shades. For instance, ethyl alcohol at times extracts only part of the color, leaving a purple hue in the tissue, while acetone frequently extracts all the color.*

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* See footnote on p. 158.

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* In addition to this paper, lantern slides were presented showing the *in vitro* reduction of NT in tumors, granulation tissue, corpus luteum of ovary, leucocytes in leukemic blood, and suspensions of tumor cells. The changes were striking under anaerobic conditions

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THE USE OF NEOTETRAZOLIUM IN THE STUDY OF *E. COLI* METABOLISM*

By HIROMICHI T. NARAHARA, HOWARD QUITTNER, LESTER GOLDMAN, AND WILLIAM ANTOPOL†

This paper is concerned with the use of neotetrazolium (NT)‡ as a tool for enzyme and cytochemical studies in bacteria. Work with *E. coli* is reported here.

Methods. The strain of *E. coli* used was isolated from a routine stool specimen. Plating the organism on Endo agar overnight produced uniformly red colonies. In our experiments, this strain was grown on enriched agar, washed, aerated, and suspended in Ringer phosphate solution buffered at pH 7.4. Experiments were set up in sterile, cotton-stoppered test tubes at 37° C. and conducted in duplicate or triplicate. 0.01 M concentrations of substrate and 1.3 mg. per cent of NT were present in the final 5.0 cc. volume of the mixture of organisms, substrate, NT, and buffer.

Results. A. NT as an Indicator of Reduction Processes. The substrates alone did not reduce neotetrazolium. NT was not appreciably reduced aerobically by washed suspensions of *E. coli* but was reduced after the addition of solutions of the following compounds, which are known to be metabolized by the organism:[1–3] *sugars*—glucose, fructose, galactose, maltose, lactose, arabinose, and xylose; *polyhydric alcohols*—mannitol, sorbitol, and glycerol; *acids*—formate, lactate, pyruvate, succinate, fumarate, and acetate; *amino acids*—glutamate, asparagine, arginine, alanine, and glycine. Lag periods were noted in the utilization of lactose, xylose, and sorbitol.

Citrate and butyrate, which are poorly utilized by *E. coli*, produced at most a trace of formazan. Dead bacteria plus substrate did not reduce NT, indicating that active metabolism is required to produce a fair degree of NT reduction. Experiments conducted anaerobically proceeded at a faster rate than the same experiments done aerobically. It must be stressed that washed suspensions anaerobically showed good reduction of NT without added substrate, in contrast to the absence of reduction by similar suspensions aerobically. This is analagous to the increased reduction already described for tissues under anaerobic conditions.[4, 5]

B. Adaptation to Substrates. The lag periods in NT reduction by lactose and sorbitol were studied. Reduction of NT began after a lapse of 1–4 hours with lactose and of about 6 hours with sorbitol. Once reduction had started, it proceeded at a fairly rapid rate. Wet preparations made when gross color had just appeared in the tubes revealed formazan granules in practically every cell.[4] No granules were discernible in specimens examined prior to the development of gross color. Growth of the bacteria in media containing the specific substrate before preparing the suspension eliminated the lag period. The incubation of the buffered substrates for 34 hours before the addition of the bacterial suspension also eliminated the lag period for sorbitol but not for lactose. The delayed reduction of NT corresponds to the absence of lactose fermentation, and apparently this phenomenon represents the period of bacterial adaptation. In the case of sorbitol, however, either adaptation or a change in the substrate by pH and long preincubation may account for absence of the lag period.

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* This paper, illustrated with lantern slides, was the second of a series of three papers presented at the meeting of the Section, February 13, 1950.

† The Joseph and Helen Yeamans Levy Foundation, Beth Israel Hospital, N. Y. and the Laboratories of the Newark Beth Israel Hospital, Newark, N.J.

‡ pp′ diphenylene bis 2-(3,5 diphenyl tetrazolium chloride). See preceding article.

C. Morphology. Formazan was seen in the cells as discrete granules without diffuse staining. In all cells and with each substrate used, these granules were seen in a subpolar location characteristic for this strain of *E. coli* and for several other strains examined but not reported here. Occasionally, smaller cells had a single granule near one end. Most cells, however, possessed one granule in each polar region, and the larger cells often had a third eccentric deposit in the midportion of the cell. As our experiments progressed, the granules slowly grew in size at approximately the same rate in all cells. In *E. coli* K-12, Lederberg,[6] using triphenyltetrazolium, observed a single large formazan granule near one pole of the cell.

Attempts to stain killed bacteria with formazan in alcohol solution or by reducing NT chemically to formazan in the presence of dead bacteria were unsuccessful. This indicates that the sites of formazan localization are the sites of NT reduction by various dehydrogenase systems. Our studies suggest the presence of organoid polar areas of dehydrogenase activity in *E. coli.* These areas may be analogous to cytoplasmic organoid structures of plant and animal cells.

Summary. (1) A large variety of organic substrates known to be utilized by the organism have been shown to reduce neotetrazolium to its colored, insoluble formazan in the presence of washed suspensions of actively metabolizing *E. coli.*

(2) Direct examination of bacteria exposed to NT and substrate allows for the detection and rough estimation of the enzyme activity of the individual cell.

(3) The lag period in lactose and sorbitol utilization is characterized by the absence of NT reduction.

(4) The regular appearance of formazan granules in the polar regions of the bacterial cell suggest that these areas are organoid and represent the sites of dehydrogenases.

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INFLUENCE OF TETRAZOLIUM SALT ON THE GROWTH AND CYTOLOGY OF ROOT TIPS*

By B. P. SONNENBLICK, W. ANTOPOL, AND L. GOLDMAN†

Neotetrazolium, a new tetrazolium derivative, has been utilized in a series of investigations concerned with root tip growth and cytology. This redox indicator, slightly yellow in color, has several advantages over tetrazolium salts previously used in studies of cellular physiology, as indicated by Antopol, Glaubach, and Goldman.[1] The stock solution of neotetrazolium (abbreviated as NT) was 0.5 per cent in strength. NT in 0.85 per cent sodium chloride gave similar results to those employing NT in tap water. The latter solution was then used, since it could be applied directly to the jars containing the growing roots. Concentrations of NT ranging from stock strength to 1 part NT in 2 million parts water were used, and *in vivo* as well as supravital (excised roots) effects were studied. Root tips of the onion, *Allium cepa L.*, were primarily employed in our tests.

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* This paper, illustrated with lantern slides, was the third of a series of three papers presented at the meeting of the Section, February 13, 1950.

† The Joseph and Helen Yeamans Levy Foundation, Beth Israel Hospital, New York, N. Y., and the Laboratories of the Newark Beth Israel Hospital and Newark College of Arts and Sciences, Rutgers University, Newark, N. J.

The most striking gross effect was the progressive color changes in the root, particularly in the active mitotic zone above the root cap. The tinctorial changes ranged from delicate pink through red to a stable purple-black coloration. The appearance of color was closely observed and it was noted that some areas within the zone showed color earlier and in greater intensity than other areas, presenting a mottled appearance. Shortly, depending on NT concentration and time of exposure, the entire meristem darkened, whereas the older differentiating portion of the root appeared unchanged or acquired a pinkish tint. A similar but not so marked effect was noted in the primary root of radish seedlings. The reduction of relatively colorless tetrazolium salts to yield colored formazan compounds and the accumulation of the latter within the embryonic plant cells result in the macroscopic changes mentioned. No gross color changes were seen after exposure to 2,3 diphenyl 5 methyl tetrazolium chloride in 0.5 per cent concentration. Reduction of 2,3,5 triphenyl tetrazolium chloride, however, can occur in excised root tips; but the effect is not so specific as with NT. With the former, the entire root is colored pink to red, and the meristem more intensely, and the red formazan begins to diffuse out of the cells by 1–2 hours after the start of a test, tinting the medium. Results with neotetrazolium are obviously not similar to those where studies with other tetrazolium derivatives have been used.

Tests were made to determine the influence on the growth of young roots of chronic exposure to NT in concentrations ranging from 11 ppm. to 0.5 ppm. (1 part NT in 2 million parts of water). With the strongest NT concentrations, color appeared earlier, but within 3–6 days such roots were behind in growth as compared with controls and also with roots given weaker NT treatments. Many such tips were abnormal in pigmentation and gross morphology. In root tips exposed to 0.5 ppm., color was visible only after 13–18 days of exposure as a diffuse but delicate pinkish tint. Growth of these tips was approximately that of the controls. In one experiment, root tips given this treatment showed better growth than those of two of four control bulbs. Dosages of from 1 part NT in 7,500 parts water to 1 in 12,000 parts (with 6–8 hours exposure) were in the sublethal zone of effect, since some of the roots, but not all, evinced subsequent growth. Tips exposed to concentrations stronger than these for the same time became flaccid and did not grow. Treatments which were lethal to developing young roots did not prevent the appearance and growth of additional roots from the same bulb, nor did such toxic root treatments influence growth of the shoots.

In sections of treated root tips, it is possible to study the orientation and appearance of the colored formazan compounds which eventuate after reduction of the tetrazolium salt. Ordinary dehydration and clearing procedures resulted in the loss of most of the dark granules, and recourse had to be made to frozen sections. It became possible, however, to demonstrate black formazan granules in thin paraffinized sections, and the method has been employed with diverse plant and animal tissues. Fixation in neutral 10 per cent formalin and dehydration in tertiary butyl alcohol is followed by several changes of petrolic ether, by a 1:1 petrolic ether-paraffin mixture, and then by embedding in paraffin. After sectioning, no cytoplasmic stain is used; the sections are either left unstained or stained only with weak hematoxylin.

The reduced substance appears within the cells as solitary black granules or aggregates of granules grouped characteristically about the nuclear membrane. While this perinuclear orientation of the formazan granules is marked in the cytoplasm, it would appear that there is no intranuclear deposit of similar granules. Following triphenyl tetrazolium treatment, stellate crystals or crystal fragments may be seen in the cytoplasm, and occasionally such crystals are located about the nuclear membrane.

The effects of neotetrazolium on nuclei were studied and some tentative comments may be made. Treatments were given in six graded concentrations (from 1:5,000 to 1:500,000). Tips were removed and fixed at successive 2-hour intervals for 24 hours, and then at 36 and 48 hours. There is no colchicine-like accumulation of metaphase figures with abnormally contracted chromosomes. With higher concentrations and increasing exposure, there is disruption of cell structure, with enhanced vacuolation and shredding of the cytoplasm to form thin, irregular strands, while after weaker treatment cytoplasmic organization persists. The nuclear effect of higher concentrations is a progressive diminution in the number of mitotic figures, the effect varying with dosage and length of treatment. The eventual picture may be either a mass of pycnotic nuclei of irregular contour or of pycnotic interphase nuclei plus an occasional prophase. At the lowest concentration used, the mitotic figure, with respect to spindle and chromosomal structure as well as the frequency of the different stages, was essentially unaltered. There is some evidence, however, that low concentrations may in fact stimulate mitoses to exceed those in control material. The influence of NT appears to be primarily cytoplasmic, and the gradual cellular alterations are probably results of interference with cellular respiratory processes.[1, 2] From the typical perinuclear concentration of reduced tetrazolium products, there may also follow modifications in cellular protein syntheses. Smear preparations of root tips show formazan deposits around the walls of the differentiating cells. Pratt and Dufrenoy[3] have reported that triphenyl tetrazolium reduction can occur about the pores of sieve tubes and other intercellular connections in the stalk of sugar cane.

The influence of temperature on the reactions leading to NT reduction within plant cells was studied. The roots were treated *in vivo*. Then they were cut and placed in stock NT solution and the reaction was timed. Time and intensity (the latter arbitrarily quantitated in degrees from zero to plus four) were recorded by two observers. Treatment at 70°C. for as little as 15 seconds resulted in no reduction of the salt. At 65°C., after 15 seconds of treatment, color intensity of the meristem of exposed root tips exceeded that of controls for approximately 20 minutes. After that they fell behind, while the controls progressed in intensity to an end point of plus four. The treated roots did not reach that end point. After 30 seconds of treatment (65°C.), the experimental tips were behind the controls in speed and intensity of the reaction from the beginning, while 90 seconds of exposure gave no subsequent color reaction. At 60°C., exposures for 15 and 30 seconds gave the experimental tips a lead in the reaction for one-half hour, after which controls and experimental material showed approximately similar responses and proceeded to a similar end point. Studies at the aforementioned temperatures, and others, lead to the conclusion that 60°C. is optimal for the reaction in onion root tips.

Tests at low temperatures have been performed (from 2°C.–10°C., and for periods ranging from 1 hour to 8 days), and it has been found that after such exposures the reaction, as determined by color intensity, is speeded up above that of the controls. The enzyme or enzymes involved in the reaction are not inhibited at the lower temperatures and speed and intensity are greater than in untreated roots. Although it was slight, tetrazolium reduction occurred even in excised roots which had been kept frozen for 30 minutes on the frozen section machine. These data permit some varied speculations.

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SECTION OF PSYCHOLOGY

AN INTRODUCTION TO THE WORK ON EXTRASENSORY PERCEPTION*

By J. B. RHINE†

We can all agree that in our search for a fuller understanding of man's nature we must overlook no source of relevant facts. We can agree, too, that the more challenging a claim may be, the more significant it is likely to be if it can be sufficiently well established.

This is all the agreement we need to take up the case for extrasensory perception or ESP (the ability to acquire knowledge of, or respond to, an external object without the use of the senses). As with any research, we must look to our methods and draw conclusions only from results that allow no alternative explanation.

For most, if not indeed for all, of those who are familiar with the great accumulation of results of ESP researches, the occurrence of ESP is established fact. Accordingly, in drawing the attention of a group still unacquainted with the field to some of the evidence of ESP, we need only to turn to records long since published, not to new or even recent experimentation. I can, in fact, think of no better methodological exhibit with which to begin than the experiment at the Duke Parapsychology Laboratory on which we based the first published conclusions that we had evidence of extrasensory perception.

This experiment, known as the "Pearce-Pratt Series," which was published in the present author's first monograph,[10] consisted of three hundred trials of card-calling tests, or 12 runs of 25 trials each. Dr. J. G. Pratt, then a graduate assistant in psychology, conducted the experiment, with a Divinity School student, Hubert Pearce, as the subject. In one building, Pratt would place the ESP-test cards face down in the center of the table in front of him at a rate of one per minute and without looking at them. In another building, 100 yards distant, Pearce would attempt to identify them as they were being laid down. Duplicate records were made and turned over to me before the two men met after the test.

The total number of hits for the 12 runs was 119. The number to be expected by chance is only 60, or five per run. The probability is less than one in a million billion that a deviation this large or larger would occur by chance alone in such a series. Such was the crucial test of ESP which led us to decide to publish, in 1934, a full report of the first three years' research. After that report, the Pearce-Pratt series was continued with a comparable scoring rate for 62 more runs, some at a greater distance and some with myself as witness. Any question concerning the data must be focused on the method used; and no essential weakness therein has so far been called to our attention.

In 1939, a group of seven experimental psychologists[1] described a set of conditions which, in their judgment, would have to be used in crucially testing for extrasensory perception. It was decided at the Duke Laboratory to meet this challenge with a special research, and for good measure a number of additional safeguards were added. This resulted in what is known as the Pratt-Woodruff research series.[9] In this work, the two experimenters, Drs. Pratt and J. L. Woodruff (who was then a graduate assistant in the Laboratory), used as subjects 32 volunteers, mostly students at Duke University, selected on the basis of interest in being tested.

The conditions of this experiment are far too elaborate for any complete description here. They require five pages of fine print in the published report, and were of such a nature that no error, inadvertent or otherwise, could be made by anyone without its being detected and either corrected or rendered ineffective in the evaluation of the results. The degree of independence in observing, checking, and evaluating results and the amount of safeguarding with locked boxes, officially stamped and numbered record sheets, and duplicate recordings surpass that of any other psychological experiment with which I am acquanited. Furthermore, the results of this elaborate experiment were definitely significant, giving a critical ratio of five. Again, no one has ever suggested any explanation but extrasensory perception.

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* This paper was presented at the meeting of the Section, February 20, 1950

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Until and unless there is some legitimate question raised regarding the quality of these experiments, there is no great point to multiplying them and certainly no need to list further samples here. I might, however, mention some further types of evidence. There are many varieties of this kind of evidence in the ESP literature.

As soon as time and attention were given to the matter, many interesting observations were made regarding the distribution of hits in ESP tests. It was found, for instance, that in certain types of tests there was a tendency to score best at the beginning of a run and to decline to a chance level half-way through the run. If the subject knew when he was approaching the end of the run, he was likely to improve and thus produce a U-shaped curve for the run as a whole (something like the memory curve). The study of these position effects led to the discovery of many similarities between the hit distributions of different researches. The most interesting of these position effects is that in which the five segments of the run (five trials in each segment) show a curve of distribution somewhat like that of the run as a whole. The composite curve of these segmental "curvelets" showed a significant covariation with the curve of the run itself.[12] Dr. B. M. Humphrey and the writer pursued this position-effect study through a number of series,[7, 13, 14] finding significant covariation present even in series in which the overall average score was not itself significant. Such wholly independent evidence of unanticipated lawfulness in the ESP data transcends the ordinary issues that have been raised by the research and leaves no alternative to the ESP hypothesis.

Still later came a somewhat overshadowing position effect of a different type, first given wide attention in the work of Carington[2] in England. I refer to displacement, or the tendency of the subject to scatter his fire, as it were, from the main target or stimulus object to the adjacent ones. Following Carington's suggestion, Dr. S. B. Soal[18] examined his own ESP records and found highly significant evidence of displacement, both to the next preceding and to the next following target cards.

Carrying this matter of displacement or "near missing" one step further brings out a rather clinching order of evidence, still a subject of current and hence unpublished research. Last year at the Duke Laboratory, Cadoret found that, in certain series of records reported earlier,[3] certain subjects tended consistently to mistake one symbol for another (*e.g.*, the circle might be regularly mistaken for the square or the cross for the star). Dr. Pratt examined the Martin and Stribic[8] series of card-calling ESP data produced by one subject and found that in the 1800 runs so far analyzed the subject not only displaced to neighboring targets to a remarkably high degree but appeared also to be consistent in mistaking one symbol for another. This analysis shows what we might call displacement both to the wrong position and to the wrong symbol, a two-way "scattering of fire."

So much for the experimental aspect of the ESP research. Little space will be used here to discuss the mathematical methods, for they are standard procedures. We have been fortunate in having the close co-operation and guidance of some of the ablest of the mathematicians of probability. For a time, Drs. Joseph A. Greenwood and T. N. E. Greville have both served as statistical editors of *The Journal of Parapsychology*. At an early stage of the ESP controversy, criticism by nonmathematicians drew from the Institute of Mathematical Statistics a statement of general endorsement of the methods of evaluation in use. Thus far, no one has found the research methods in error to an extent that altered the conclusions drawn in any case.

We have, of course, been taking ESP as an established reality for over fifteen years In that time, some progress has been made beyond the mere conclusion itself, though by no means anything like what could have been achieved had we been able to go on with the research without fighting a long battle over the question of the occurrence of ESP, or if there had been accessible to us the financial resources for research that are available to any recognized branch of psychology. These limitations should be kept in mind in considering some of the progress made toward understanding the nature of ESP.

First of all, we find that ESP is very much an unconscious operation. The subject has no awareness as to how he experiences the symbol on the card or what happens before he decides whether to put down a star or a circle. Introspection provides him with no reliable information. He may have his own special way of selecting the symbol. He may hallucinate the symbol on the wall, he may take the first image that comes into consciousness, he may have a verbal impression, or he may have no idea as to how he is making his choice.

It is a significant fact that, when Dr. B. F. Riess's[15, 16] subject scored over 18 hits per 25 over a 74-run series and later, after an illness, scored at only the chance average of five, she had no idea that she was doing any differently in one case than in the other. Here is the main source of the difficulty, not only in the research on ESP, but in making any practical use of the capacity. So long as the subject is unaware as to how his ability works and when his impression is received, he can have no reliable conviction as to whether he is right. This unconsciousness naturally is a great barrier to the development of the ability. It is difficult to improve without awareness of success. In a few exceptional cases and in certain spontaneous experiences, however, it is clear that certainty is possible. We do not yet know the necessary conditions, but the discovery of them is now one of the main goals of the research.

We can appreciate the handicap of this unconscious nature of ESP better when we realize that it carries with it not only the uncertainty as to rightness and wrongness, but that it is equally likely to lead the subject to avoid the target (*i e*, unconsciously but systematically to miss it or reject it). This might be due either to unconscious negativism or merely to wrong method in making responses. Any consistent following of a mistaken method would lead to a negative deviation from mean chance expectation. A subject might, in the second half of his test session, go into some kind of a perverse mood, or shift his tactics without realizing he was doing it, thereby getting negative, not just chance, results and wiping out all the positive results given during the first half. It is obvious, then, that we have this "sign factor" to deal with in an ESP test, as well as the amount of the ability itself. Two subjects with equal ESP ability, one with a somewhat positive turn and the other with a negative twist, would cancel each other out if they did the same number of runs.

Dr. Gertrude Schmeidler, of the College of the City of New York, has found repeatedly[17] that, among her subjects, those classified as "sheep" (the ones willing to accept the idea of ESP) give ESP scores averaging above mean chance expectation, while subjects designated as "goats" (those who reject the possibility of ESP) average below the expected mean. This separation has repeatedly given significant differences. When Dr. Schmeidler, using Rorschach responses as evaluated by the Monroe Inspection Method, makes a further breakdown of her sheep and goat subjects who have taken the ESP tests, she finds that her highest scoring sheep are those that are both well-adjusted and nonconstricted, while goats with these same characteristics give the largest *negative* deviation. This Rorschach separation looks more like one dealing directly with the amount of ESP ability itself; but it is not necessarily so. It is likely that a number of complex personality operations are involved in ESP, and all these would naturally have their own conditions.

Another of these methods of separating high and low scorers was discovered by Dr. Humphrey,[4] of the Parapsychology Laboratory at Duke, in her use of Elkisch's diagnostic technique based on the form quality of a subject's drawings. Dr. Humphrey found in some preliminary tests that the "expansiveness" or "compressiveness" of the subject's drawing was associated with his ESP score, and she concentrated upon this criterion alone. On the basis of these ratings of drawings, she has now had repeated success in separating high and low scorers when the ESP test itself was based upon a drawing of a concealed target picture. To some extent, the same trend has been found when the ESP test was based entirely upon the card-test method with the drawing only accessory. Dr. Humphrey's method, like the sheep-goat classification, separates the subjects who score above expectation from those who score below. In the simple card-calling test, the compressives tend to give a negative deviation and the expansives a positive one. The amount of ESP ability, however, may very well be the same, and "sign" the only variant.

The late Dr. C. E. Stuart,[19, 5] also of the Duke Laboratory, introduced an interest test which was given to the subject for response as to likes and dislikes. On the basis of the subjects' responses, Stuart worked out a scale of rating in which those expressing the greatest number of strong likes or dislikes were called "extremes," and those who were more moderate were called the "midrange" group. By dividing the subjects into midrange and extreme groups, he found that on a criterion series he could separate those giving positive deviations from those giving negative deviations. Applying the method to sixteen new series, with the help of Dr. Humphrey, he produced a significant separation of the high- and low-scoring subjects in these series taken as a whole. More recently,[6] Dr. Humphrey has extended the discrimination value of the Stuart Interest Inventory by the development of a 14-item scale, which gives a still better division of the subjects of the same sixteen series into high and low scorers and on an independent principle of separation.

The combination of the expansion-compression criterion and the two interest criteria, which are independent of each other, gives a separation that is better in terms of average scores than that of any two of the three methods used. Naturally, however, as the research advances, we hope to move much closer to the point where we will be able to say what kind of states and traits of personality are associated with high or low scoring in the ESP tests. The construction of special tests for the selection of the personality best fitted for good performance in ESP tests is under way.

It is not possible to review here the studies made on ESP to find its relation to space and time. These studies have, for the most part, already been published or soon will be; and a fairly up-to-date review may be had from the book, *The Reach of The Mind.*[11] These tests of the relation of certain mental processes to the external physical environment are, in my judgment, by far the most important of the researches in parapsychology. They cannot, however, be fully appreciated in brief review. Moreover, they are extremely revolutionary to the current materialistic philosophy which prevails in the sciences. Accordingly, these experiments on ESP in time and space may well be left out here, and reference made to them (in the pages of the *Journal of Parapsychology*) for those readers whose interest is sufficient for a further, and indeed larger, step outside the restrictive lines of conservative psychological doctrine.

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DIVISON OF MYCOLOGY

ISOLATION OF CULTURABLE FUNGI FROM DIVERSE HABITATS*

By B. M DUGGAR†

In the sense here employed, the "isolation" of a fungus has reference to the preparation of that type of culture now generally known as a pure culture; that is, to the establishment and active growth in otherwise sterile, nutrient media of a single strain or species of fungus, free from any other living organism. The presence of any other organism would constitute a "contamination." The term "isolate" may be applied to any form or species thus segregated. The general purpose of isolation is, of course, to study or utilize this particular organism uninfluenced by any other biotic effects. In the case of many culturable fungi, such cultures as those just referred to are of first importance in order to proceed with any intensive studies. I shall try to be more specific by enumerating a few examples of this need.

As a likely procedure in tracing accurately the structural and developmental characteristics—the life history—of the organism, such cultures may be essential and will always be helpful.

A pure culture is a requisite if the fungus is to be studied in respect to its nutritional requirements or in determining the significant relations and responses of the organism to temperature, light, or other environmental factors. The plant pathologist, and indeed the human pathologist and dermatologist, may find it important for diagnostic purposes and, of course, a source of inoculum both for tests of pathogenicity, when these are desirable, and for *in vitro* tests in relation to fungicides and therapeutic agents.

Usually, the pure culture is indispensable in fermentative work in which various products of metabolism are collected and chemically isolated, as in the production of antibiotics. In certain brewing processes and dairy-product manufactures, however, the pure culture may be essential only as a starter, the fermentation being carried out otherwise with less consideration for sterile materials. Obviously, the metabolic products of the fermentation may soon limit contamination.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Division, February 24, 1950.

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Respecting high lights in the development of biological isolation as a standardized procedure, I shall limit the discussion drastically. Directly upon the heels of improvements in heat sterilization of media and utensils, during the latter part of the past century, the problems of isolation appeared in the pathway of both bacteriologists and mycologists. As early as 1873, Klebs described a "fractional" procedure. About five years later, Lister laid the foundation, I would say, for the "dilution" method, which, in one form or another, constitutes our major technique of today. The earlier applications of the method were burdensome, but not impossible. Essentially, the program was this: material containing, or supposed to contain, cells or spores of the organism was diluted with sterile water to such an extent that a drop of the final suspension would contain, on the average (and preferably) not more than a single cell. It may be inferred that the Lister procedure, if accurately carried out, would be tedious in the extreme. In further development, the higher dilutions were also spread lightly over the surface of a sterile slice or block of potato, so that with growth the colonies were often widely separated one from another. Thus, solid media came into the picture.

About 1881, Koch's leveled plate technique was introduced, the medium being first gelatin and later agar. That was the beginning of a new era. At this point, one should be reminded, however, that, with gelatin and agar available as solid media, the Esmarck rolled tube appeared as a modification of the Koch procedure. Six years after Koch's paper, Petri suggested a further variation from the plate procedure, using instead the covered dish which is now a central item in almost any isolation program.

In order to bring my remarks regarding current usage in isolation procedures within the limits of time and space available, and probably more in line with current interest, I shall omit any consideration of isolation as applied to anaerobic or marine conditions or to many other specialized types of environment. The following outline will suggest some of the procedures requiring brief attention:

A. The dilution series and some modifications.
    (1) The routine dilution series with nutrient agar.
    (2) Dilution with elimination of the heat-sensitive.
    (3) Dilution with change of pH of the agar.
    (4) Dilution with elimination of the antibiotic-sensitive.
    (5) Dilution with other agents differentially deleterious.
    (6) Dilution with dye indicators.
    (7) Dilution after "enrichment" of the substrate.
    (8) Dilution with changes in the nutrient agar.
    (9) Dilution with flooding.
B. Special sources of spores or of other growth-competent cells or tissues.
    (1) Promotion of spore formation.
    (2) Exudation of gelatinous spore masses.
    (3) Spore discharge from sporangia, perithecia, apothecia, *etc.*
    (4) Special cases in plant and animal pathology.
C. The direct transfer method.
    (1) The case of the fleshy fungi.
    (2) The case of rapidly invaded host tissues.
D. The single-cell isolation (not here considered).
E. Special procedures with the water molds or aquatic fungi.

In practice, the routine dilution series with nutrient agar may be greatly modified by the mycologist, plant pathologist, dermatologist, or bacteriologist, depending upon, among other considerations, (1) the nature of the materials from which isolations are to be made, (2) the general characteristics of the organisms sought or expected, and (3) the population density and diversity. Assuming that the material consists of samples of garden, agricultural, forest-land, and grass-land soils and that the objective is the isolation of common molds and Actinomycetes, no great departure need be made from the routine performance except as it may be expedient to attempt, at the same time, the elimination of certain heat-sensitive organisms, especially bacteria. In this case, the procedure would involve two parallel dilution series, one with and one without the application of heat.

With adequate soil samples, 4–5 grams of soil are placed in a test tube and this is shaken vigorously after adding 15–20 ml. of distilled water. Allowing 10 minutes for sedimentation, and using a sterile pipette, about 10 drops from the midsection of the supernatant are transferred to a tube of sterile physiological salt solution (or sterile tap water). This constitutes tube 1, from which eventually about 5 drops are transferred to plate 1 of the 3-plate dilution series. From tube 1, at any dilution desired, tube 2 is prepared, and so on for tube 3, each of these tubes serving as inoculum for the corresponding Petri plate. As soon as the tubes have furnished inocula for the regular series, and if no duplicates were arranged, they are heat-treated, commonly at 53° C. for 15 minutes. In general, incubation is at 28°C., and 4–6 days may be required before transfer to agar of Streptomyces colonies is practicable.

In order to promote diversity among the colonies appearing on the isolation plates, the agar for the four plates may be adjusted to any pH desired within favorable limits for any species sought. Parallel series may be arranged at different pH values.

The development of antibiotics has made possible the use of these antimicrobial substances, at least those with strongly differential potencies, to exclude certain classes of bacteria from the plates to the advantage of certain molds, yeasts, and Actinomycetes. Aureomycin at a concentration of 0.25γ or more per ml. has been notably helpful. Penicillin is useful where the flora is made up of many Gram-positive spore formers. In such situations, the antibiotic is a new tool.

Other toxic substances may be serviceable in special cases. Norris has shown that common species of *Chaetomium* have a tolerance for calcium hypochlorite so high that the use of this agent greatly simplifies isolation. Dye materials and certain substances displaying visible oxidation or reduction changes have been used in bacteriology chiefly for indicator effects, but some dyes may be serviceable also in differential isolation.

The preparation of dilution cultures after enrichment of the substrate (for example, of the soil), by the addition of special nutrients known to promote the growth of organisms desired, has been practiced to advantage in soil bacteriology, but it appears that enrichment has been used by mycologists chiefly through the use of highly complex decoctions or infusions, as in the use of manure decoction to promote isolation of coprophilous fungi. Obviously, any important modification of the nutrient agar, arranged to be used in comparison tests with the standard agar, might be selectively serviceable.

In the several preceding paragraphs it is assumed that the isolation inoculum is eventually diffused in the agar of the plate. It is always possible, however, in each case separately, to flood the plate with the inoculum, thus disposing the spores on the surface of the agar. When so arranged, the dilution should be such that the liquid quota for each plate should be raised to about 18 drops (*ca.* 0.9 ml.), and the surplus liquid, after flooding, should be absorbed with sterile filter pads. If the spores are recognizable, when thus spread on the surface, germination and early transfers of individual spores may be made. A modification of the above is to smear separate drops of the inoculum on areas marked from below, so that observation may be simplified.

In any preparation of inocula for isolation series, every consideration should be given in advance to the promotion of spore formation of the specimen. Commonly, this is attained by a pretreatment which permits the control of moisture and temperature. Adequately moist material in slowly ventilated vessels at a fairly uniform temperature, and with some control of light, are conditions easily available to all.

In the case of many fungi growing upon herbaceous or woody plants, with spores produced in densely stromatic areas or within pycnidia, the mycologist often has access to spores in mass. With favorable moisture, the spores may be exuded as gelatinous horns, as in the chestnut blight fungus; or in masses, as in many anthracnoses. Such material is often nearly free of contaminants.

The forcible discharge of spores from or in sporangia, from perithecia (asci), and from hymenial surfaces has long been known. This field has been closely investigated in recent years by Buller. For present purposes, the interesting mechanism is cited as affording opportunity for catching the spores on sterile slides or cover glasses, excellent material for dilution series, or even for direct transfer to agar slants, with types representative of the three major classes of fungi.

Many special cases arise in the experience of pathologists, both on the plant and on the animal or human side, in which a satisfactory inoculum is difficult to achieve. Often it may be essential to reinoculate the host or a trial host, in the hope of effecting an elimination of secondary organisms. In the establishment of pure cultures of fleshy fungi, it is often reasonable to assume (and has indeed been shown) that direct transfer, aseptically, of small fragments of tissue from a region of growth to prepared media may be the quickest route to satisfactory stock cultures for any practical purpose.

Somewhat analogous to the procedure indicated in the previous paragraph is that often used when an invading fungus grows up through active tissues of the host. Cultures of the corn smut fungus, *Ustilago zeae*, may be obtained readily by direct transfer, after aseptic excision, of tissue containing the fungus. Vascular or cortical parasites (represented, for example, by *Fusarium* sps.) of herbaceous hosts may be transferred directly, if the host tissue is sufficiently large and the exact region of new growth of the invader definitely located. In such cases, usually, surface sterilization by some hypochlorite is an aid.

Procedures with aquatic or certain moist soil fungi belonging to several families of the Phycomycetes, which may be expressed as Saprolegniaceae and allies, are, or may be, essentially dilution techniques. However, the early treatment of the material is often that of casting a pinch of soil (presumably containing some spores) into a small vessel of water, then attempting to "trap" motile spores in this weak soil suspension by throwing in a few seeds of hemp or other fragments of organic matter. Later, if the hemp seeds have served their purpose, it is possible to transfer mycelium and sporangia to fresh containers, so that eventually satisfactory inoculum is obtainable.

It is perhaps apparent that the foregoing brief text could not be intended as a comprehensive guide to the biological isolation of the fungi. The material is offered rather as suggestions respecting current practices, limited, yet sufficiently extensive, it is hoped, to promote interest in a field which is basic to further studies of fungi.

SECTION OF ANTHROPOLOGY

ARCHAEOLOGICAL RESEARCH IN AFGHANISTAN*

By WALTER A. FAIRSERVIS, JR.†

The vast interior region of Asia, the so-called "Inner or Central Asia," has had a strong influence from very ancient times on the high culture centers at its fringes, *i.e.*, India, China, and the Near East. At present, the extent and nature of this influence is largely speculative, because political as well as geographical barriers have effectively impeded thorough scientific investigation of the critical regions. The tendency has been to regard "Central Asia" more as a geographical link, over which passed cultural elements from one "high" culture center to another, than to visualize the area as possessing, within its bounds, centers capable of developing to a high degree certain cultural traits now accredited to other regions. What these traits might be is also speculative, for obvious reasons; but it is felt that such traits as the use of iron, tanning, horse domestication, the compound bow, tailoring, certain bronze-making techniques, and numerous others, are of Central Asian origin.

If "Central Asia" can be understood to include all the region from Manchuria to the Danube in Rumania and from southern Siberia south to the northern mountains of Baluchistan and the Himalaya, it is apparent that there are geographical points within this region which do not conform to the usual, desert-mountain-steppe conception of the area. Such watered regions as the deltas of the Oxus and Jaxartes, the slopes of the northern Altai, the valleys of the Don and the Volga, the slopes of the Ural mountains, and the basin areas such as Seistan and the Tarim are and were suited to agricultural and technological development on a large scale. These areas have either abundant rainfall or a dependable water-table, and the possibilities of supporting an agricultural population outside the nomad pattern are excellent. Failure by archaeology to gain sufficient access to these critical areas, however, has left their true historio-cultural status unknown.

For the reason, then, that these regions cannot be validly generalized as mere bridges between cultures in the face of geographic suitabilities for non-nomadic culture growth, it was and is felt that archaeological research in Central Asia is of great importance for an understanding of the true picture of the development of the high cultures of Asia. With the world political situation preventing access to most of the region concerned, the kingdom of Afghanistan, with its lenient-to-the-West attitude, becomes the obvious area for this necessary research. This is further enhanced when one considers that the Helmand River Valley and the Seistan basin are both within the bounds of Afghanistan.

Whereas the historical records do concern themselves somewhat with certain parts of the Afghan area, *e.g.* Bactria. Drangiana, Arachosia, Sakistan, *etc.*, the prehistory of the country is a complete blank. If however, one considers not only the palaeolithic finds of de Terra, Paterson, and others along the Indus Valley and in Peninsula India, but also the neolithic sites of Iran (Hissar, Sialk, Giyan, *etc.*), Baluchistan (Sur Jangal, Rana Ghundai, Nal, *etc.*), and the Indus Valley (Mohenjo Daro, Haruppa, *etc.*), with their tantalizing traces in painted pottery and other artifacts of cultural inter-relationships, then Afghanistan (and especially Seistan) becomes critically important.

It was with these problems in mind that the First Afghan Expedition of the American Museum of Natural History went to Afghanistan. The initial enterprise was carried out as a survey, with a view not only to test the original theories, but to prepare the way for possible large scale excavation next season. The survey had three principal aspects, which are described in the following paragraphs.

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* This paper, illustrated with standard and Kodachrome slides, was presented at the meeting of the Section, February 27, 1950.

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The first was a geological investigation of the Helmand River Valley along a 200-mile stretch divided into halves at the Girishk line. This was supplemented by excursions into the Dasht-i-Margo, west of the Helmand. No glacial terracing was found in the river valley, but tremendous layers of boulder conglomerate were found overlying a reddish sandstone. This sandstone was found free of the boulder conglomerate some twenty miles into the desert. The age of these formations cannot as yet be ascertained, but the implication of the studies made by Fox and others in Central Afghanistan indicates that the sandstone is Cretaceous or Early Tertiary and that the boulder conglomerates are of later age but probably not Pleistocene. The extent of these gravels some 20 miles on either side of the present river valley indicates that in Tertiary times the Helmand had cut a much larger valley, which was filled in with the gravels resulting from the erosion of the still growing Koh-i-Baga ranges. The present Helmand, confined in its channel, has cut down into this gravel bed to the sandstone base. Overlying this sandstone base is a modern floodplain which is extensively cultivated by the present-day Afghans.

The lack of glacial evidence in this area indicates that the mountains to the south and east prevented moisture deposition on the southern slopes of the Koh-i-Baba and thus impeded the growth of mountain glaciers. This ice-free region may have been, as a consequence, an area suitable for the existence of man and animal. However, no artifacts of Palaeolithic time have yet been recovered.

The second aspect of the expedition's work was the concentration on the locating of sites of the so-called "Chalcolithic" period. These usually take the form of tells. In Seistan, over 35 tells were located and the surface collections made at each of them, while predominantly of Islamic types, have revealed at least one prehistoric site. The climatic conditions encountered prevented a wide search of the area, but it is certain that there are a number of these sites in Afghan Seistan just as there are in Persian Seistan.

In the Quetta Valley, just south of Kohjak Pass, a tell was visited that contained sherds similar to those of Stein's sites at Dabar-Kōt, *i. e.*, a polychrome ware of red and black on red with geometric designs of triangles and lozenges. This valley has a number of large tells scattered here and there which, with the exception of those visited by Piggott in the immediate vicinity of Quetta, are unreported. The promise for future results in this region is therefore very good.

The third aspect of the expedition's work involved a number of small projects which may open the way for full study in the future:

(1) The discovery and photographing of a large number of petroglyphs, which are found wide-spread over the country. On the whole, these are similar to those reported by the Russians from the Pamirs.

(2) The initiating of an anthropological study of the Afghan people. This involved observation, measurement, and photography. Over 50 subjects were studied, and it is hoped that this series may be continued in the future by a physical anthropologist.

(3) A brief visit to the Islamic city of Peshawarun, which lies deserted in Seistan. This is one of a large number of such places in the Seistan basin. It was visited by Goldschmidt, McMahon, Hackin, and Ghirshman but was only briefly reported. The interest to the First Afghan Expedition lay in its extraordinary demonstration of the failure of the water-table in the area. It is an extensive ruin stretching some 6½ miles by 5 miles, with many of the inner walls in excellent preservation. A number of wood and other organic specimens were collected in hope that both Carbon 14 and botanical tests may cast more light on this question of water-table shifts in a limited space of time.

The relations of the expedition personnel with the Afghans were of the best. Both the government and the Afghan people were very cooperative, and the hopes for future work are therefore very good.

Our survey is inconclusive, of course, as most surveys are. It is felt, however, that it has crystallized some of the problems sufficiently for future research in Afghanistan to have every chance for maximum accomplishment.

CONFERENCE HELD

SECTION OF BIOLOGY

"THE GROWTH, REPLACEMENT, AND TYPES OF HAIR"

FEBRUARY 10 AND 11, 1950

The Section of Biology held a Conference on "The Growth, Replacement and Types of Hair." Doctor James B. Hamilton, Long Island College of Medicine, Brooklyn, N. Y., and Mr. Amos E. Light, The Wellcome Research Laboratories, Tuckahoe, N. Y., were the Conference Chairmen in charge of the meeting.

The program consisted of the following papers:

Friday, February 10

Morning Session. Chairman, C. P. Leblond, Department of Anatomy, McGill University, Montreal, Canada.

INTRODUCTION AND GENERAL DESCRIPTION

"Role, Significance, and General Structure of Hair and the Piliary System. The basic Histological Structure of Hair, with a brief Comparison to Feathers, Nails, and Other Appendages," C. P. Leblond.

MORPHOLOGY AND DISTRIBUTION OF HAIR IN MAMMALS

"Morphology and Phylogeny of Hair in Mammals," C. R. Noback, Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Patterns and Distribution of Hair in Mammals," Jerome P. Parnell, Department of Anatomy, Long Island College of Medicine, Brooklyn, N. Y.

"Types and Distribution of Hair in Man" S. W. Garn, Department of Anthropology, Harvard University, Cambridge, Mass.

NORMAL DEVELOPMENT, GROWTH, AND REPLACEMENT OF HAIR

"Development of the Piliary System and the Replacement of Hair in Mammals," E. O. Butcher, Department of Anatomy, New York University School of Medicine, New York, N. Y.

"The Hair Cycle of the Mouse and Its Importance in the Study of Sequences of Experimental Carcinogenesis," S. B. Wolbach, Children's Hospital, Boston, Mass.

"Mitotic Measurements of the Rate of Proliferation of Epidermis and Associated Structures," C. P. Leblond and W. F. Storey, Department of Anatomy, McGill University School of Medicine, Montreal, Canada.

Afternoon Session. Chairman, Mildred Trotter, Department of Anatomy, Washington University School of Medicine, St. Louis, Missouri.

"Rate of Growth, Replacement, and Life Cycles of Hair in Man," R. J. Myers, Department of Zoology, Colgate University, Hamilton, New York, and James B. Hamilton, Department of Anatomy, Long Island College of Medicine, Brooklyn, New York.

"Changes in Morphology of Hair During Childhood," Mildred Trotter and O. H. Duggins, Jr., Department of Anatomy, Washington University School of Medicine, St. Louis, Missouri.

"Appearance of Adult Patterns of Hair in Man," E. L. Reynolds, Samuel Fels Institute, Antioch College, Yellow Springs, Ohio.

"Quantitative Measurements of a Secondary Sex Character, Axillary Hair," James B. Hamilton, Department of Anatomy, Long

Island College of Medicine, Brooklyn, New York.

"Physiological Role of the Piliary System in Mammals and Its Relation to the Environment," L. P. Harrington, John B. Pierce Foundation, New Haven, Connecticut.

CHEMICAL AND PHYSICAL PROPERTIES OF HAIR AND THEIR RELATIONSHIP TO STRUCTURE

"Chemical Classification of Keratins," R. J. Block, Department of Biochemistry, New York Medical College, Flower and Fifth Avenue Hospital, New York, New York.

"Histochemistry and X-Ray Diffraction Studies," A. Giroud, Department of Histology, University of Paris, Paris, France, and C. P. Leblond, Department of Anatomy, McGill University School of Medicine, Montreal, Canada.

"The Results of X-Ray Diffraction Studies on Keratin Fibers," R. S. Bear and H. Rugo, Department of Biology, Massachusetts Institute of Technology, Cambridge, Massachusetts.

Saturday, February 11

Morning Session. Chairman, E. O. Butcher, Department of Anatomy, New York University School of Medicine, New York, New York.

"Polariscopic Observations," S. M. Garn, Department of Anthropology, Harvard University, Cambridge, Massachusetts.

"The Role of the Hydrogen Bond in the Structure of Wool," P. Alexander, Wolsey Ltd., Abbey Park Mills, England.

FACTORS INFLUENCING GROWTH, LOSS, AND REPLACEMENT OF HAIR

"Inheritance," L. W. Thigpen, Baptist Memorial Hospital, Memphis, Tennessee.

"Minute Structure of Hair and Its Correlation with Morphology, Physiology, and Behavior," C. E. Keeler, Georgia State College for Women, Milledgeville, Georgia.

"Nutrition," M. L. Morris, Raritan Hospital for Animals, New Brunswick, New Jersey.

"The Effect of Folic Acid Antagonists on Hair Growth," Sidney Farber, Children's Medical Center, Boston, Massachusetts.

"Irradiation," F. P. Ellinger, U. S. Naval Medical Research Institute, Bethesda, Maryland.

"Effects Produced by Atomic Bomb," A. A. Liebow, Department of Pathology, Yale University School of Medicine, New Haven, Connecticut.

"The Relationship of the Adrenal Cortex, Pituitary, and Thyroid Glands to the Growth of Hair," B. L. Baker, Department of Anatomy, University of Michigan School of Medicine, Ann Arbor, Michigan.

"Testes and Ovaries," G. Van Wagenen and W. U. Gardner, Departments of Obstetrics and Anatomy, Yale University School of Medicine, New Haven, Connecticut.

LOSS AND FAILURE OF GROWTH OF HAIR

Afternoon Session. Chairman, Marion Sulzberger, College of Physicians and Surgeons, Columbia University, New York, New York.

"Shedding of Hair in Mammals as a Normal Occurrence," L. W. Thigpen, Baptist Memorial Hospital, Memphis, Tennessee.

"Patterned Loss of Hair in Man. Incidence and Types," James B. Hamilton, Department of Anatomy, Long Island College of Medicine, Brooklyn, New York.

"Patterned Loss of Hair in Man. Pathogenesis and Prognosis," Amos E. Light, The Wellcome Research Laboratories, Tuckahoe, New York.

"The Non-Patterned Alopecias (Alopecia areata, Congenital Alopecia, Febrile States, *etc.*)," C. L. Laymon, Division of Dermatology, University of Minnesota, Minneapolis, Minnesota.

EXCESSIVE DEVELOPMENT OF THE PILIARY SYSTEM

"Hirsutism," G. W. Bissell, Department of Medicine, University of Buffalo School of Medicine, Buffalo, New York.

"Role in Cancer of the Skin," N. C. Foot, Department of Pathology, Cornell University, Ithaca, New York.

"Round Table Discussion of the Piliary System from the Standpoint of Therapeutics"—*Chairman:* Marion Sulzberger, College of Physicians and Surgeons, Columbia University, New York, New York.

1. "Use of Physical and Chemical Agents to Promote Growth of Hair," Marion Sulzberger.
2. "Irradiation," E. M. Uhlmann, Michael Reese Hospital, Chicago, Illinois.
3. "Treatment of Fungus Infections, Baldness, Hirsutism, and Other Pathological States," S. Rothman, Department of Dermatology, University of Chicago, Chicago, Illinois.
4. "Dangers Involved in the Waving of Hair and in the Use of Cosmetics and Dyes," J. Downing, Boston, Massachusetts.
5. "Medicolegal Aspects," B. M. Vance, Office of Chief Medical Examiner, New York Department of Health, New York, New York.

The Section of Biology provides conferences for active workers in the special fields of Biology.

Attendance is limited to those invited to participate in these conferences and to interested members of the Academy.

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Elected February 23, 1950

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SER. II, VOL. 12 *April, 1950* No. 6

TRANSACTIONS
OF
THE NEW YORK ACADEMY OF SCIENCES

Editor
ROY WALDO MINER

Associate Editor
WEBSTER BRIGGS

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Executive Director: EUNICE THOMAS MINER

THE DELAWARE AQUEDUCT: SOME GEOLOGICAL DATA*

By THOMAS W. FLUHR†

The Delaware aqueduct system, part of which has been in operation for a number of years, comprises several reservoirs and tunnels extending from the Catskill Mountains to New York City. The general geologic features of the aqueduct are well known and are on record.[1, 2] This discussion will be limited to a few of the geologic features which appear to be of scientific interest. It will not attempt to settle any of the controversial hypotheses concerned with the correlation of various geologic formations or to be dogmatic about the data it presents. I have already altered my views on some controversial issues more than once and hope that I shall continue to do so as new evidence becomes available.

Deformation South of the Highlands. The geological formations of New York City extend in a northeasterly direction to the southern margin of the Highlands belt. They lie in straight regular belts as far as Katonah. At that place, a major fault runs in a southeasterly direction toward Bedford Village. East of Katonah, there is a rugged mass of gneisses and granite, much of which is included within the Poundridge Reservation of the Westchester County park system. Beginning at Katonah, the formational belts, which up to this point follow a straight northeasterly course, turn to the east and swing in great arcs around the Poundridge block. To some extent, the southeastern margin of the Highlands follows the same pattern.

The general trend of the rock formations extending from New York City to Katonah is northeast, and the dips of the foliation in almost all cases are to the northwest. Folding is isoclinal, with the axial planes tipped to the southeast. From Katonah to the margin of the Highlands, the trend changes from northeast-southwest to east-west, and the rock structure dips northwest and north. The southeastern margin of the Highlands is a fault, for the Highlands block has been uplifted, and nowhere within the Highlands massif are there any traces of the Inwood and Manhattan formations. At Somers, the Highlands gneisses have been overthrust on the Inwood, the contact plane dipping to the northwest beneath the Highlands. Within the Highlands, in the area between the West Branch reservoir and Somers, the rock structures similarly trend northeast-southwest or east-west and dip to the northwest or north.

The entire rock structure between the West Branch reservoir and New York City suggests a thrust exerted by the Highlands massif, with movement directed in a southeasterly direction. North of Katonah, the thrust has not only crumpled the formations between the Highlands and the Poundridge block, but has displaced them toward the southeast with development of the major fault, which extends from Katonah toward Bedford Village. In the aqueduct tunnel, a repeated pattern is found, an overthrusting movement from the northwest with a large vertical component and a smaller horizontal component. The northwest side is displaced upward and to the east, accompanied by drag of the foliation structure and shearing.

The significance of the structure, whereby the formational belts loop in great arcs around the Poundridge block, is not understood.

Correlation of the Gneisses. All geologists working in this region concede that the gneisses of the Highlands belt are pre-Cambrian, for in the vicinity of Fishkill the Cambrian Poughquag quartzite overlies them unconformably. Whether they are to be correlated with the Grenville is a question on which the author has no basis for personal opinion. There are various intrusives into the gneisses, some of which must be much younger. Most local geologists correlate the Fordham gneiss with the gneisses of the Highlands. This correlation seems to be based merely on the fact that both are gneisses. A study of both indicates that the basis for the correlation is rather uncertain.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, March 6, 1950.

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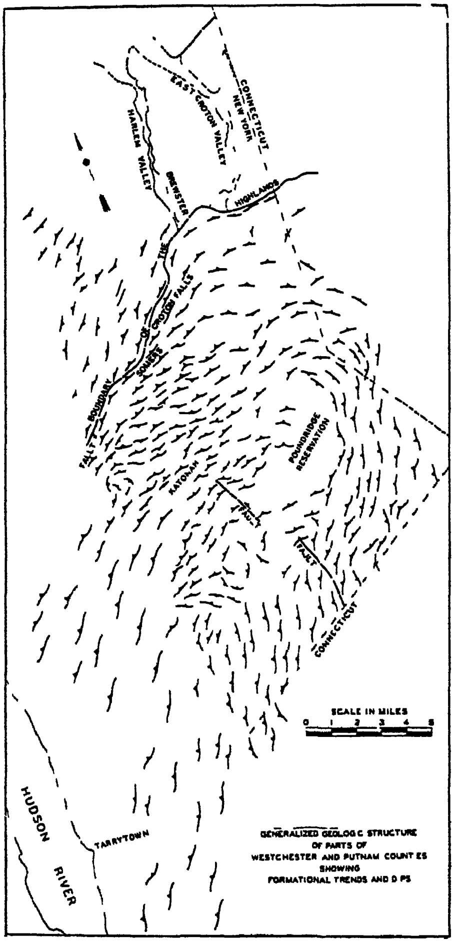

Figure 1. (Reproduced by Courtesy of the Delaware Supply News).

The Fordham gneiss extends from New York City to near the southern margin of the Highlands in long narrow well-defined belts. Although secondary metamorphic structures have been developed in it, there are many places where there are indications of primary structures, such as original bedding. Such structures may also be found in the gneisses of the Highlands, but usually are less well-defined, less constant in direction, and indicative of a much higher degree of dynamic metamorphism. Structures in the Highlands belt suggest that the rocks have undergone plastic flow to a greater degree than those of the Fordham.

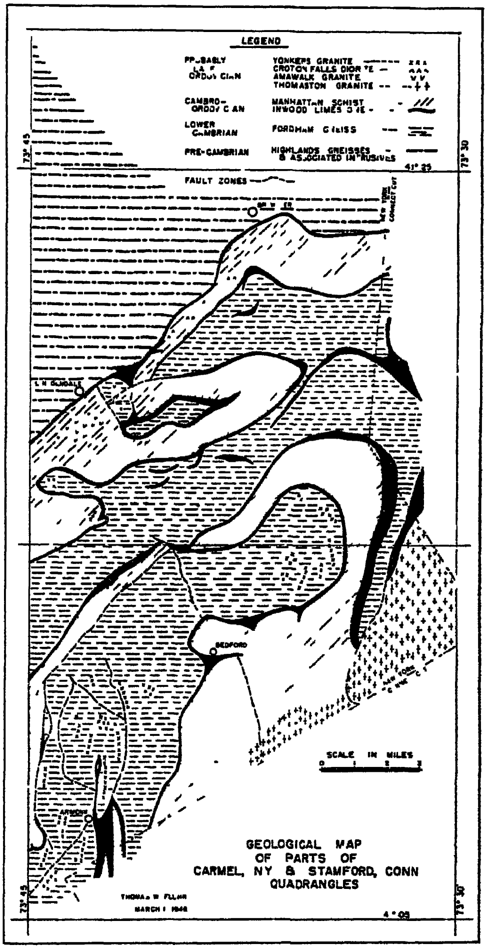

Figure 2.

The mineralogic composition also differs. The composition of the Fordham and whatever traces of original bedding remain suggest that it was originally a series of sand stones. The country rock of the Highlands, at least as far as my own observations have gone, appears to have been a graphitic schist, with some discontinuous, lenticular, distorted, highly silicated limestones. Iron ore deposits occur frequently in the Highlands but are absent from the Fordham. Granitic intrusives into the Fordham are usually characterized by orthoclase and microcline feldspars, while those of the Highlands carry andesine and other basic feldspars.

The dynamic characteristics of the formations likewise differ. Wherever the Fordham has been faulted, crush zones remain as evidence. Displacements in the gneisses of the Highlands are frequently characterized by plastic flow, with no remaining evidence of crush zones. Much of the faulting in the Highlands evidently has occurred at more deep-seated levels than in the Fordham.

These facts are not proof of anything, of course, but they suggest that the Fordham gneiss is much younger than the gneisses of the Highlands. The evidence which proves that the gneisses of the Highlands are pre-Cambrian is not necessarily applicable to the Fordham. In fact, it may not be pre-Cambrian at all.

Correlation of the Manhattan-Inwood Series. Whether the Manhattan-Inwood series should be correlated with the Hudson River-Wappinger series is a problem which has plagued every serious student of local geology. My own opinion on the subject has varied from time to time. Without reiterating the various opinions pro and con, I may state that the evidence of Dr. Robert Balk, based on very detailed field work, in support of the hypothesis that the two series are equivalent, is very impressive. The one stumbling block is the fact that he has not been able actually to walk out a connection between the two.[4, 5] Geologic work in connection with the Delaware aqueduct has not shed any additional light on the problem. It is sufficient to state that the close similarities between the series support Dr. Balk's views.

One major objection to the adoption of Dr. Balk's ideas has been the supposed conformability of the Inwood limestone with the underlying Fordham gneiss,[6, 7] another controversial issue on which I must confess various changes in opinion.

Eleven contacts of the Inwood limestone with the Fordham gneiss were crossed by the Delaware aqueduct tunnel, and one contact of the Inwood with the gneisses of the Highlands belt. Naturally, in view of the opportunity for securing evidence of conformity or unconformity, I examined each of the contacts with great care, in some cases even bringing in banks of flood lights in order to illuminate them. I must admit my disappointment. Those contacts where crush zones occurred were, of course, useless for the purpose. In others, there was found a parallelism of structure. To my mind, it is not sufficient to find a parallelism of structure; it must also be possible to show that the structures in both formations represent original bedding. This I was unable to do. Where folds exist in a metamorphic formation, it is sometimes reasonable to assume that they represent traces of original beds. Where the structure is merely parallel, there is no proof as to whether it is primary or secondary. For example, in Central Park are many outcrops of Manhattan schist. In many places, minor folds are to be seen, pitching toward the southwest. Often, even in the same outcrop, the folds disappear and the only structure remaining is a steeply dipping schistose foliation. One may suspect with some reason that the folds are evidence of bedding, but one must conclude that, where the folds disappear and are replaced by a steeply dipping schistosity with all layers parallel, the original bedding has been totally obliterated. Similar structures are to be found in the Inwood and Fordham formations.

The structures within the limestone, parallel to the contacts crossed by the tunnel, were defined mainly by micaceous layers and by layers of diopside. These may be secondary structures developed by metamorphism; hence, in my opinion, they are valueless as a proof of bedding. There were also indications of plastic flow in the limestone near the contacts, and, in one place, angular schist xenoliths, evidently sheared off from somewhere. Moreover, the gneiss and limestone formations frequently were separated by quartz or pegmatite veins, which often follow zones of displacement.

The contact of the Highlands gneiss with the Inwood limestone is of special interest. The gneiss has been thrust upward and over the limestone, the fault plane dipping at sixty degrees to the northwest beneath the Highlands massif. In spite of the evident faulting, the two formations are tightly squeezed together, the limestone pressing into every wrinkle and fold of the gneiss, with no evidence of crushing. Here, where displacement of undetermined, but evidently great, magnitude has occurred, the parallelism of the foliation of both formations with the contact is even better developed than elsewhere. Therefore, one may readily understand why my opinion of parallelism of structure as a criterion of conformability suffered a severe setback.

In this connection, attention may be called to an interesting situation in New York City. In the vicinity of Featherbed Lane and Jerome Avenue, there are outcrops of the Fordham gneiss. The structure is that of an open fold: a syncline with its axis near the end of Davidson Avenue, with a corresponding anticline farther to the east near Jerome Avenue. Both plunge to the south. An extension of the belt of Inwood limestone, whose western margin is generally followed by Jerome Avenue, here forms a re-entrant and overlies the syncline of gneiss, while the anticline of gneiss pitches downward beneath the limestone. Unfortunately, the limestone is not exposed. Its presence was determined by borings, one of which penetrated entirely through the limestone in the syncline. As is usual in New York, the outcrops are insufficient in number to prove anything beyond question of a doubt. The gneiss outcrops, however, are suggestive. The parallelism of the minor fold structures with the synclinal and anticlinal major structure and with the embayment of limestone looks remarkably like original bedding and probable conformity of the two formations. Again, however, the conformity cannot be regarded as proven.

Interbedded Limestone. Limestone layers occur within the Fordham gneiss formation. The presence of such layers is evidence favoring the sedimentary origin of the Fordham. To assume that such layers, as now found, represent the original bedding in other than a very crude way is an unwarranted conclusion. The tunnel driven north from Shaft 15, near Mt. Kisco, passed through such a limestone layer of large size. In it were many xenoliths of hornblendic gneiss, similar lithologically to the variety of gneiss surrounding the limestone layer. The xenoliths varied in size, were well spaced in the limestone, and were angular. In form and distribution, they did not resemble a talus or other sedimentary deposit, but rather appeared to be of autoclastic derivation. Studies of the shear structures suggested that the region had been subjected to two deformations, during which fragments of the gneiss had been torn off and incorporated within the limestone, which under the dynamic stresses had behaved as though it were plastic.

The evidences of plastic flow of the limestone and its incorporation of xenoliths indicate that in this case the deformation was of sufficient degree to obliterate the original bedding structures.

If the Fordham gneiss and Inwood limestone are to be regarded as conformable, as the author believes, in spite of the inconclusive character of the contacts crossed by the tunnel, and if the Fordham gneiss and the gneisses of the Highlands are not related, then a way is cleared for reconciling the various views which have been expressed in regard to correlation. It appears to the author that there is at least some reason for regarding the Manhattan-Inwood-Fordham group as a conformable series of Paleozoic age. The precise correlation I will leave to the stratigraphers.

The Quartzites. No mention has as yet been made of the Lowerre quartzite, a formation which has been described in the literature, but which I must admit I have never seen. There is an outcrop called the Lowerre at Hastings-on-Hudson; but I have never been able to satisfy myself that it is a quartzite. The term "quartzite," I would limit to a rock of sedimentary origin, composed of quartz grains cemented together with silica. I am not satisfied that the term has always been used in this sense in local geology. In connection with work on the Delaware aqueduct, it was necessary to map the Poughquag quartzite near Hortontown. Some of it obviously is a quartzite, carrying pebbles. Some of it is fine grained, without pebbles. As the mapping progressed, however, I found myself following the "quartzite" into the gneisses. Since the two are unconformable, evidently something was wrong. Mr. Paul Bird and I went over the area again and collected samples, from which Mr. Bird made thin-sections. This solved the problem. Some of the material is Poughquag quartzite. Some of the fine-grained material, looking very like quartzite, proved to be a sheared and recemented granite. We thereupon collected samples from various localities where quartzite had been mapped and found that some of these likewise were not true quartzites.[8] I wonder if the Lowerre is perhaps one of these "pseudoquartzites."

Nothing new or startling has been revealed by excavation of the Delaware aqueduct. It has to some extent raised more questions than it has settled, and I leave these for the consideration of the geologists working in the metropolitan area.

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SECTION OF BIOLOGY

GENETIC MECHANISM CONTROLLING BACTERIAL RESISTANCE TO STREPTOMYCIN*

By M. DEMEREC†

For several years we have been investigating the origin of bacterial resistance to antibiotics and have studied patterns of resistance to penicillin, streptomycin, chloromycetin, neomycin, and aureomycin. In this work, we have used strains B and K-12 of *Escherichia coli*, and (in the case of penicillin, to which *E. coli* is resistant) *Staphylococcus aureus.*

When samples consisting of a known number of bacteria are taken from a broth culture of *E. coli* and plated on broth agar containing various concentrations of streptomycin, it is observed that, as the concentration increases, the number of surviving bacteria that are able to form colonies becomes smaller and smaller. If the logarithm of the percentage of survivors is plotted against concentration of streptomycin, the curve obtained (FIGURE 1) shows four characteristic features. The horizontal first portion of the curve (between 0 and 1 unit per ml.) makes it evident that streptomycin does not become active until after a certain threshold concentration has been reached. After that point, even slight increases in concentration greatly reduce the percentage of survivors, as indicated by the sharp slope of the second portion of the curve (between 1 and 3 units). In the third portion (between 3 and about 15), the slope changes and becomes more gradual; whereas, the last part of the curve (after 15) is parallel to the base line. If the bacterial material were uniform with regard to resistance to streptomycin, the curve would presumably continue as a straight line until it met the base line (see the broken line in the figure). The difference between this hypothetical broken line and the actual curve suggests that a small fraction of the bacteria, represented by the area of the difference, is more resistant to streptomycin than the rest of the population. The horizontal fourth section of the curve shows the presence in the population of a few bacteria that are entirely resistant to streptomycin.

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† Department of Genetics, Carnegie Institution of Washington, Cold Spring Harbor, New York. This study was conducted in part under a grant from the American Cancer Society, through the Committee on Growth of the National Research Council.

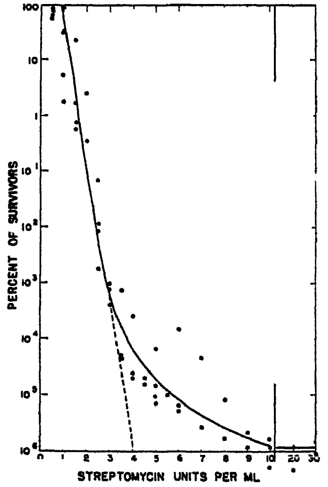

FIGURE 1.

Very similar survival curves are obtained in experiments using bacteria from different cultures, suggesting that the observed behavior is a general characteristic of bacterial resistance to streptomycin. As a rule, strains established from colonies that grow on media containing streptomycin remain either completely or partially resistant.

This complete or partial resistance, detected when bacteria are thus brought into contact with streptomycin, might conceivably be induced by the antibiotic. On the other hand, it might be due to a genetic change, entirely independent of the action of the streptomycin. There is a method available for determining which of these two possibilities is true. Experiments made to test this point indicate that resistance to streptomycin originates through genetic changes, and that the streptomycin acts only as a selective agent, eliminating sensitive bacteria and permitting resistant individuals, which are always present in a large population, to reproduce and form colonies.[1]

The mutation process that is responsible for the presence of a small number of resistant bacteria in any large population occurs with a low frequency of about once per one billion. Because experiments can be carried out with very large numbers of bacteria, however, precise determinations of such low mutation rates can be made. For example, it is possible to put on a single Petri dish one billion or more bacteria, and, if the medium contains an adequate amount of streptomycin, all the sensitive individuals on the plate will be eliminated, whereas the resistant ones will survive and form colonies. In this manner, minute proportions of resistant bacteria can be measured with a great deal of accuracy.

Among the streptomycin-resistant mutants, two types can be recognized: those that are simply resistant, and those that are dependent on streptomycin for growth. In strains B and K-12 of *E. coli*, these two types occur with a frequency of 40 per cent and 60 per cent, respectively. When bacteria of a dependent strain are plated on a medium not containing streptomycin, they will usually pass through a few divisions and then die out. If any nondependent variants are included among them, however, or should originate during the few divisions undergone before death, these individuals will be able to grow on the medium without streptomycin. Thus, a method is available for detecting mutants from streptomycin dependence to nondependence; and, here again, large numbers of bacteria may be used and low mutation rates measured. Therefore, making use of changes to streptomycin resistance and dependence, and reversions to nondependence, it is possible to study and to determine the frequency of mutations occurring in two directions, a situation that is still unique in genetic research.

The mutants that arise in a dependent strain are also of two types, namely, streptomycin-resistant (though not dependent) and streptomycin-sensitive. From the sensitive reversions, it is again possible to isolate "second-order" mutants that are resistant to or dependent on streptomycin; and, from these new dependents, a second order of sensitive reversions may be obtained. Thus, the process of mutational change may be continued through many orders.

A study was made of various properties, aside from streptomycin susceptibility, of different mutants derived in this way. It was found that some differed from one another in rate of growth, others in sensitivity to ultraviolet radiation, in rate of mutation, in proportion of different mutants yielded, or in having or not having a nutritional deficiency. Some of the dependent mutants differed in being able to grow on a medium containing one part or another of the streptomycin molecule instead of complete streptomycin. Considering all these differences, it seems likely that a large proportion of mutants that are unrelated in origin are different from one another. This would indicate that a reversion from streptomycin dependence to sensitivity may be due not to a reverse change in the chemical reaction that produced the original mutant, but to some other reaction, which re-establishes the original physiological conditions in the cell.

Using the K-12 strain of *E. coli* and methods developed by Lederberg,[2] attempts were made to obtain recombinants between several resistant and dependent mutants. The fact that none were observed suggests that the changes involved in the mutational processes determining bacterial resistance to streptomycin occur in a single gene-locus.

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DIVISION OF MYCOLOGY[*]

COCCIDIOIDOMYCOSIS[†]

By R. C. BURKE[‡]

The historical background of coccidioidomycosis is one of the most interesting in the field of systemic medical mycology. That this is true is due not only to the early misconception of the disease as being protozoan in etiology but even more to many original discoveries made by physicians and mycologists, most of which were shown later to be applicable to other systemic mycoses or their etiologic agents.

First among these, of course, was the

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[*] The Section of Psychology held a meeting on March 20, 1950, at which a paper was presented by Karl D. Kryter entitled, "Psychological Research in the Air Force's Human Resources Research Laboratory." No abstract of this paper has been received.

[†] This paper, illustrated with lantern slides, was presented at the meeting of the Division, March 24, 1950. Another paper on the same subject, by J. H. Swartz, was also presented at the meeting. No abstract of this paper has been received.

[‡] Research Associate, Biological Laboratories, Harvard University, Cambridge, Massachusetts. This work was financed in part by a grant from the Ella Sachs-Plotz Foundation and was carried out at the University of California at Los Angeles in partial fulfillment of the requirements for the M. A. The author wishes to express her gratitude to Prof. O. A. Plunkett for his helpful advice.

demonstration by Ophüls and Moffit (1900) that coccidioidomycosis was caused, not by a protozoan, but by a fungus which possessed a dual nature, appearing in host tissues and exudates as a spherical organism but in culture as a fluffy white mold. Likewise, the source in nature, though long suspected for some systemic pathogens, was established first for *Coccidioides immitis* when Stewart and Meyer (1932) repeatedly isolated the fungus from soil collected in the San Joaquin Valley, California. Furthermore, the first evidence of the possible existence of reservoir hosts was pointed out for *Coccidioides* by Emmons (1942), who noted that in endemic areas of the disease certain species of rodents were infected with the fungus. Also, the first indication of the dual nature of some of the systemic mycoses (with exogenous sources) was noted in the case of coccidioidomycosis in 1937 by two physicians, Gifford and Dickson, who proved that, in addition to the already known grave, generalized, and highly fatal type of coccidioidomycosis termed by Dickson (1937) as "progressive" coccidioidomycosis, there was also a much more common, mild respiratory type, which Dickson called "primary" coccidioidomycosis.

These examples can be carried further—into the realm of serology, followed by the first extensive use of a skin-testing antigen in epidemiological studies of the systemic mycoses. The antigen from *C. immitis*, known as coccidioidin, is prepared and administered in much the same way as tuberculin. Results of skin-testing surveys with this antigen have helped to define the endemic areas present in the southwestern region of the United States and have disclosed that within these areas most of the long-time residents have had coccidioidomycosis. These studies have also shown that, in many individuals, these infections have been sub-clinical.

The Disease. As mentioned earlier, there are two types of coccidioidomycosis. The symptoms of the primary type usually appear 8–14 days after the inhalation of dust-borne spores and consist, primarily, of a low grade fever and nonproductive cough. Newcomers to the area frequently have the more severe manifestations of an influenza or pneumonia-like infection. In most cases, this mild respiratory type disappears within several days to two weeks In some cases, however, usually less than 1 per cent, secondary progressive coccidioidomycosis develops. The first evidence of dissemination within the patient usually appears within a few weeks after onset of the disease and often manifests itself as a persistence of the primary pulmonary infection coupled with an extension of the disease within the lungs. In other instances, the primary pulmonary lesions may or may not heal, the first evidence of progressive infection being either the formation of large sacculate subcutaneous abscesses, cutaneous granulomatous ulcers, draining sinuses from bone lesions, or involvement of the central nervous system. Any organ of the body, with the usual exception of the gastro-intestinal tract, may be involved. In about 50 per cent of the cases of progressive coccidioidomycosis, the infection is fatal, death ensuing after from a few weeks to several months.

Mycology. Diagnosis is made by the laboratory demonstration of the fungus and its isolation in culture. In tissues and exudates, the fungus appears as a thick-walled spherical body ranging from 15–80 μ in diameter, but averaging about 30 μ in human cases. These spherules, as they are commonly termed by members of the medical profession, are sporangia and contain cytoplasm, which at first is homogenous and then becomes more granular as the sporangia mature. Finally, the content cleaves into endospores, which are released by rupture of the sporangial wall.

When pus, sputum, or exudate containing spherules is planted on Sabouraud's glucose agar and incubated at room temperature or at 37°C., the sporangium germinates and develops quickly into a moist membranous colony. In 10 days to 2 weeks, a typical colony is composed of a greyish-white mycelium divided into three zones: a central tuft, a middle zone of sparse mycelium, and a peripheral zone of cottony mycelium. In older cultures, the colonies frequently become buff-colored.

A microscopic mount of the aerial growth of a colony reveals the presence of hyaline, rather closely septate hyphae, 2–4 μ in diameter, with occasional racquet cells and with chains of barrel-shaped to spherical chlamydospores separated from one another by empty hyphal elements. Emmons (1942) has noted that the first chlamydospores forming in culture appear on specialized mycelial branches, the identity of which is lost in older cultures. The chlamydospores are highly infectious. The fact that they are produced in large numbers, are dry, and are easily dissociated from the mycelium makes handling of old cultures dangerous. Few individuals work with the mycelial stage of the fungus for an extended period of time without contracting primary coccidioidomycosis.

Handling the spherule stage is less dangerous, since the sporangia and endospores are formed in moist tissues and exudates and are not airborne. Unfortunately, no reliable and consistent method exists for culturing sporangia *in vitro.* Lack (1938), using a medium composed of glucose broth and partially coagulated egg albumen, demonstrated the production of sporangia under anaerobic conditions, but unfortunately, this method has not proved dependable. MacNeal and Taylor (1914), Dickson (1937), and the author have observed the production of spherical structures in mycelium in pus. Since these structures remain attached to the mycelium, and never have been observed to form endospores, we must conclude that they represent unsuccessful attempts to produce the sporangial stage. The fact that the spherule stage is able to propagate itself in or at the edge of pus deposits in the animal host but is unable to continue reproduction when the pus is removed from the host might suggest the need of an *in vivo* factor for consistent sporangial production. This, of course, indicates the use of experimental animals, a practice we should like to avoid. There is no doubt that the use of experimental animals has been particularly satisfactory in clinical studies of host reaction and, to a large extent, in studies of the morphology of the spherule and endospores. Their use has been less satisfactory, however, for the study of the development of the cultivated chlamydospores into sporangia. Even more, the necessity of having to rely on experimental animals has prevented many studies both in the field of mycology and in the field of medicine.

In Ovo *Culture of Sporangia.* It is obvious from the foregoing that there is a need for a consistent and reliable method for culturing the sporangial stage *in vitro.* Such a method would not only permit studies of the physiology of the spherule stage, it would also, no doubt, make possible the obtaining of more clear-cut serological reactions. Likewise, this method would enable systematists to work more easily with the fungus and also allow medical personnel to work with the spherule stage rather than with the highly infectious and dangerous mycelial stage.

In an effort to find an *in vitro* method of culturing the sporangial stage, many types of culture media and environmental conditions were tried by the writer, but with no success. It was finally decided that perhaps the use of "living culture tubes" might provide at least a partial answer to the problem of culturing the sporangial stage. "Living culture tubes," in this instance, are defined as living organisms which provide *in vivo* conditions yet remain inert, presenting, in this respect, *in vitro* conditions. Among the various possibilities presented, fertile chick eggs seemed best suited for the purpose. Later, it was discovered that Moore (1941) had cultured the sporangial stage on the corio-allantois. This present work is extended, however, to include embryo, allantoic fluids, air sac membrane, albumen, *etc.*

The first experimental inoculations revealed that the sporangial stage reproduced in fertile chick eggs in massive numbers. The results were so spectacular that it was decided to investigate some of the problems, conditions, and results of this work. Much remains to be done, but, from this preliminary work, it is evident that the use of fertile chick eggs offers an almost unparalleled opportunity to study sporangia seemingly unaffected by host reaction. In carrying out this work, the procedure was as follows: (1) dip in alcohol and flame 9–12-day fertile chick eggs; (2) remove section of shell with hand drill; (3) inoculate spores of *C. immitis* to chorio-allantoic membrane; (4) seal window with sterile glass coverslip or cellophane and paraffin-glycerine mixture (4:1); (5) place eggs in 37°C. incubator, turn daily (do not obscure window), and candle to determine if alive.

In order to minimize the danger of handling mature cultures of *C. immitis*, the following technique for inoculation in step 3 above was developed. Using a hypodermic syringe and needle inserted through cotton plug, physiological saline is ejected onto the culture slant. The chlamydospores, easily washed free, rise to the surface. About 0.25 cc. of the suspension is drawn into the syringe and used for inoculating the chorio-allantois.

Results. In a series of 177 fertile chick eggs inoculated between the 5th and 16th days of incubation with *C. immitis*, a mortality rate of 95.48 per cent was noted. Two series of controls have indicated, however, that a certain percentage of these fatalities were due to both normal death expectancy and to mechanical injury of blood vessels. The fungus was isolated or identified in tissues in 81.5 per cent of the inoculated eggs. The remainder can be accounted for by the early deaths of some embryos due to tearing of a blood vessel when opening the shell, to 4.52 per cent of chick hatchings, and to difficulties involved in selecting infected tissues in embryos autopsied after the 12–14th day. Actually, it makes little difference when the fertile chick eggs are inoculated.

In most cases, there were no obvious signs of infection of any part of the embryo. Nevertheless, the fungus was shown to be present in the sporangial stage in tissue sections of embryo and extra-embryonic membrane and in mounts of yolk sac and albumen. Massive numbers may be found in the allantoic fluids. In addition, the mycelial stage frequently could be found on the membrane of the air sac. The extent of the infection, and consequently the distribution of the sporangia in the embryo, appears to depend upon the length of time the chick survives, survival being influenced by at least three factors: age of chick when inoculated, size of inoculum, and damage to membrane. Even though study of this aspect of the problem has not been completed, the presence of the fungus in blood vessels of the chorioallantoic membrane and in the ventricle of the heart, as well as the demonstration of sporangia in lung and liver of one chick which was too weak to complete hatching, indicate that the infection may be widespread through the embryo.

That the chick is an inert host is indicated by several factors, which include massive infections, the rapid growth, the reproduction, and the very large size of some of the sporangia. The inert character of the chick egg is made even more evident when it is compared with the relative nonsusceptibility of young, newly hatched chickens. Intraperitoneal inoculations of *C. immitis* into 7 young chicks resulted in no obvious infections. In only two of the chicks sacrificed at the end of four weeks were a few sporangia found in the lungs and liver.

Comparison of Sporangia from Fertile Chick Egg, Human Cases, and Experimental Animals. Comparison of sporangia cultured in fertile chick eggs with those found in man and in experimental animals reveals several differences. In the first place, massive numbers of sporangia are obtained very often in chick embryo cultures. These sporangia, for the most part, and particularly in the case of embryos less than 13 to 14 days old, appear to be lying free in the fluids of the embryo chamber. Considerably smaller numbers are found in embryo tissue. That the fungus develops rapidly and almost unrestrained is indicated not only by the large size of some of the sporangia, which range in size from 3–240 μ, but also by rapid and prolific formation of endospores. In fact, most sporangia are in some stage of endospore production. In human cases and also in experimental animals, such vast numbers of cells are not seen. In general, the diameter of the fungus in human pus, exudate, or tissue varies from 15–80 μ in diameter, with most averaging about 30 μ; sporangia in human sputum and in mice average somewhat larger. Likewise, most of the sporangia in human cases are in an immature state.

Sporangia cultured in chick embryos show morphologic variations more frequently than in human cases and in experimental animals. One of these is a frequent variation from the spherical shape so characteristic of sporangia found in human and animal hosts. Another is the relatively thin wall which, in sporangia from fertile chick eggs, is only about 1 μ, while, in human cases and in experimental animals, the wall averages 2–4 μ in diameter. It seems possible that this thin wall may result from rapid growth of the fungus and the lack of host reaction, which might act to stimulate the production of a heavy wall. The presence of this thin wall may account for the variety of shapes of the sporangia, especially those which are caught in rapidly developing chick tissue, packed in dense clusters, or crowded with endospores.

In addition to this variation of shape, there are other and more spectacular morphologic differences, which must be considered as abnormal if we designate the form and structure of the sporangia in human cases and in experimental animals as "normal." These "abnormalities" include sporangia with pseudopod-like structures and other extrusions, some of which appear almost bud-like. In addition, there are a few sporangia with heavy cross-walls giving a two-chambered appearance and there are spherules incompletely separated from one another. Most of these changes probably result from rapid growth in an inert host, whereas others may be due to a variety of unknown factors. It is believed that a study of these morphologic variations might give added insight into the still unsolved systematic position of this fungal pathogen.

None of the sporangia grown in fertile chick eggs was observed to contain a large central vacuole. Occasionally, in human cases and more frequently in mice, cells containing large vacuoles are evident. Emmons (1947) has suggested that large size and large vacuoles are indicative of rapid growth. Both rapid growth and large size are two of the outstanding characteristics of sporangia grown in fertile chick eggs. The absence of large vacuoles then, might suggest that the physiological properties of the host may be of more importance.

One of the most striking features in all of the sections examined is the variable staining reaction of the sporangia. With the use of Mallory's triple stain, the spherules stain either brown, blue, yellow, orange, or red in color. No correlation of staining reaction with size or with maturity was noted. Also, location of the fungus in the embryo or egg had little influence, a possible exception being those cells found in albumen and in blood vessels. The sporangia in albumen were consistently among the largest organisms noted; most were in an immature state and stained a blue-brown color. Those sporangia in blood vessels were much smaller, frequently showed signs of endospore production (even in those cells which were no larger than red blood cells), and usually stained a brown color. Endospores, with few exceptions, took the same stain as the parent sporangium; however, occasional red endospores were found within a blue or brown parent cell. The variability of the staining reaction needs to be investigated further. The work by Alhfeldt (1929), who also obtained a variable staining reaction when she used osmic acid and Van Gieson's stain, suggests that lipoidal content may be of some importance in this respect.

Mycelial Stage in Chick Eggs. In examining the eggs for pathologic conditions, a curious phenomenon was observed. In 30.9 per cent of the 177 inoculated eggs, *C. immitis* was found growing in the mycelial form in the air sac. Embryos which were inoculated on the 6th to 8th days showed this phenomenon in about 50 per cent of the cases. This percentage steadily decreased with the age of embryos inoculated until, on the 16th day, no eggs showed the mycelial form on the air sac membrane.

The earliest appearance of the fungus in the air sac began as a whitening of the membrane in an egg inoculated 48 hours previously. Older colonies are small (5–8 mm. in diameter), plaque-like, irregular, and composed of compacted mycelia which, when examined microscopically, show few if any chlamydospores. The absence of chlamydospores may be due to the youth of the colonies examined, or it may be due to slow growth in an unfavorable environment. It is not known whether the mycelium derives its nourishment from the membrane, which is composed of organic fibers, or from the chorio-allantois directly under the air sac membrane. Likewise, the exact way in which the fungus finds its way to the air sac is not known. It seems possible, however, that some of the fungus cells which penetrate the blood vessels of the chorio-allantois are brought to the air sac membrane. This might account for the tendency of the mycelial stage to appear near the center of the membrane.

Discussion. The fact that it has been demonstrated that the sporangial stage of *C. immitis* can be cultured in fertile chick eggs raises the possibility of applying this technique to the solution of other problems. As an example, using the technique of Taylor and Carmichael (1948), drugs can be injected between the shell and chorio-allantoic membrane to test their effectiveness for treatment of coccidioidomycosis. In addition, since large numbers of organisms are present in the allantoic and amniotic fluids, it may be possible to collect them for use in serological as well as physiological studies. There is, also, the possibility that immunological studies utilizing this technique may result in production of a vaccine which can be used to protect newcomers to endemic areas.

Summary

(1) The procedure and results of inoculating fertile chick eggs with *C. immitis* are described.

(2) That the use of fertile chick eggs is an excellent method for obtaining large numbers of sporangia for study is indicated by the massive infections which result and by the large size and rapid reproduction of the sporangia.

(3) Sporangia are found throughout the egg: in chick embryo, extra-embryonic membranes, albumen, yolk sac, and, particularly, in the amniotic and allantoic fluids. The mycelial form sometimes occurs on the air sac membrane.

(4) A comparison of the morphology of the sporangia noted in fertile chick eggs with those found in human hosts and experimental animals showed a greater variation in size, shape, abnormal variations, and staining reaction in those sporangia grown in fertile chick eggs.

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SECTION OF ANTHROPOLOGY

ICONOGRAPHIC STUDIES AS AN AID IN THE RECONSTRUCTION OF EARLY CHIMU CIVILIZATION*

By GERDT KUTSCHER†

For an appraisal of the ancient Peruvian Civilizations, two main sources of information are available. One is the written reports and records of the Spanish Conquistadores and christianized Indians, such as Guaman Poma de Ayala, and the other, the archeological discoveries and excavations, which are especially rewarding in the coastal regions. These two sources stand in marked contrast to each other when viewed in relation to the Incaic and Pre-Incaic periods. While the written records for the Incaic culture are abundant, the art forms and representations are relatively poor. A reconstruction of the Incaic civilization which would rely solely on the archeological finds would be a rather difficult task. Here, the early chronicles fill out the gap which would exist otherwise.

The situation is quite different for the reconstruction of the Pre-Incaic civilizations of the northern Peruvian coast. The number of reports recorded in early Spanish times is very small and can almost be counted on the fingers of one hand. One should mention the unique and extremely valuable report by Antonio de la Calancha on the civilization of the late Chimu, which can be gathered from his "Crónica Moralizada." Besides Calancha, there are Cabello de Balboa and an anonymous author in the year 1604. The historical importance of these sources has only recently been properly estimated by John Rowe.[1] On the whole, however, the information given by these writers on the Pre-Incaic civilizations is extremely poor. On the other hand, there is an immense amount of archeological material, obtained mainly from the very rich burial finds, material which gives an insight into the civilizations which created it.

This is especially true of the ceramics which were produced by the early Chimu or Mochica. This coastal civilization, which might have flourished in 600–800 A.D., had its main centers in the valleys of Chicama and Moche. Its northern borderline might have been the valley of Pacasmayo and its southern border, Nepeña. To Max Uhle, we owe the first stratigraphical work, which made it possible to fix approximately the historical position of the early Chimu civilization in the sequence of cultures of the northern Peruvian coast. Max Uhle called this civilization "Proto-Chimu" because of its "primordal character." A. L. Kroeber and P. A. Means used the term "Early Chimu," which seems appropriate, since there are many common links between this relatively early period and the Chimu period (called "Late Chimu" by Kroeber). This late Chimu can be considered as a sort of renaissance, after an interregnum or intermediate period of foreign styles.

What the people of the early period called themselves will never be found out, but it is quite certain that they did not call themselves "Mochica." This name, given by the old chroniclers, designates the language spoken by the Chimu. To this author, the application of this strictly linguistic term as a name for the early period does not seem too well justified. It might be more convenient to select a neutral term, such as "Civilization of Moche" or "Mucheros," rather than to call them "Mochica." So long as no neutral term has been proposed, we are here calling them "Early Chimu," as Kroeber and Means did.

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The early Chimu, as was shown by Max Uhle for the first time by his excavations in Moche, made thousands upon thousands of ceramics, painted in a characteristic bicolor scheme, reddish brown on cream or molded to represent entire figures, animals, human heads, *etc.*

The extraordinary artistic and esthetic value of these ceramics, especially of the so-called "effigy" vessels in the form of human heads, has been appreciated very early by connoiseurs. This esthetic value is paralleled, however, by its importance as documentary source material. The vessels can be considered, thanks to their ornamentation, as a singular source for the reconstruction of the civilization of these people. The unique position of the early Chimu pottery rests on two facts: they are unique in form as well as in content. The form is extremely realistic, portraying an enormous variety of topics. Nothing seems to have been too low, vulgar, or revolting, nothing too high or sublime, that it was not depicted.

At first sight, it seems almost impossible to get any order into this extreme variety of topics. Almost everything seems to have aroused the interest of the sculptor and painter of that time. Animals and plants, hunting and fishing, war and trade, dances and ceremonies, all are shown. Of special interest is the large number of representations of their gods and demons.

The extraordinary importance of these objects as anthropological documents has been appreciated since the time of E. G. Squier. Even in our day, however, these works have too often been used as a means of illustration for reports on the Incaic period. Direct interpretations of this kind, based on mere analogy, is not free from dangers and pitfalls, as two examples may show.

(1) Incaic religion was centered around worship of the sun. On the other hand, Antonio de la Calancha tells us that the Chimu of the late period considered the moon their supreme deity, which was called Si. This god, regarded as the deity of all crops and the boisterous sea, was venerated in a big "huaca," called Si-An, the "house of the moon." Opposed to the Incaic sun-worship, the coastal people stuck to the worship of the moon. This might be explained by the climatic conditions in this part of the country.

(2) Another example is furnished by the difference in the importance attributed to the fox by the Incaic and by the Pre-Incaic coastal civilizations. The fox (as well as the skunk) was despised in the Incaic time, as the myths of the region of Huatochin show, whereas he had a very important position in the highly developed religious systems of the coastal peoples. The jaguar, Tello's "dios felino," so important in the Chavin civilization, is no equal to the fox in Early Chimu religion and mythology.

These two short examples show quite clearly that it is not possible to apply ceremonies, ideas, customs, *etc.*, reported to have been used by the Inca, by mere analogy to the Pre-Incaic civilizations, which were highly developed in their *own* way.

It seems more plausible not to assay these civilizations by mere analogy, but to understand them out of themselves, *i e.*, by their own works—by the way they have depicted themselves.

Among the great number of Pre-Incaic coastal civilizations, the Early Chimu seems the most propitious for such a task for three reasons, two of which have been mentioned already: the realistic style and the wide scope of representation. The third reason is the large number of ceramics which have found their way into the museums of both America and Europe. To supplement the ceramics in this study, there are the buildings and the other contents of the rich grave finds: objects of stone, bone, metal, shell, and, more rarely in this period, textiles.

It is, therefore, rather tempting, with all this aid, to make an effort to classify and group this variety of representation and to try to reconstruct this ancient civilization. Iconographic studies afford the means for a cultural analysis which finally should lead to an understanding of this culture as a whole.

E. G. Squier was the first to recognize the value of the archaeological findings (especially the ceramics) as an aid in reconstructing this civilization. Twenty years later, Arthur Baessler became interested in the deeper meaning of the effigy vessels and vase paintings which he had collected and later published in his monoumental work on "Ancient Peruvian Art" in 1902–03. This study, however, was not a systematic one. Gösta Montell's excellent work on "Dress and Ornaments in Ancient Peru," which also dealt with the Early Chimu has not been followed by similar studies in other fields of culture. Of the gigantic work begun by Rafael Larco Hoyle, only two volumes have been published so far.[2] The present author, intrigued by the possibilities, has already attempted a further systematic reconstruction of the Early Chimu civilization.[3]

The enormous amount of material seemed to make it necessary to limit the iconographic study at first to selected, special aspects of this civilization and to study them more or less separately. In collecting the material, however, it became clear that this method could not be used, for the same reason that it cannot be used in modern social anthropological field work: the different aspects of the culture are related and connected with each other in the most intimate way. The warfare was, as we will see later, connected with religious ideas, while, on the other hand, an analysis of the demons and gods requires a study of the different animals, plants, *etc.* Therefore, it became inevitable to regard these representations more or less as parts of a whole and to consider these interrelations. This seems to be the only way to come to an understanding of the whole system of thoughts and ideas which gave origin to these representations.

Before giving a few practical examples to show how these iconographic studies work out, a few words should be said about the character of the ceramics.

The Early Chimu used three techniques for ceramic decoration with equal mastership; plastique, high relief, and painting. By "plastique" is meant that the vessel was cast in the form of a human being, a human head, an animal, *etc.* These works are more or less sculptured in the round, and only the stirrup-spout reminds us that they are vessels in practical use. The vessels decorated with reliefs and paintings in white and reddish brown form another group.

The easiest approach, of course, is found in the effigy vessels, which quite correctly are considered to be in the first row of ancient American art. They may also be regarded as anthropological specimens which yield a great deal of information. First of all, they tell us about the physical type of these people in a most naturalistic and sincere way. A comparison shows that this early civilization was built up out of a mixture of at least three different racial types. The physical appearance of the Early Chimu is therefore better known to us than that of most other Indian tribes. The vessels further inform us of the headdresses, the ear ornaments, and the facial paintings.

The same holds true, to an even greater degree, of the vessels which have been shaped in the form of an entire human being or an animal. They tell us about the costumes, ornaments, implements, and weapons used in this period. Usually, they depict a single sitting or standing figure, with little or no mobility (Plate 17[3]). On the other hand, there are occasionally groups which depict action: on the top of one vessel, a medicine man is treating a sick person stretched out before him (Plate 40[3]). Another vessel shows a group of three persons, a motif very popular in Early Chimu but also repeated in the late period: a drunkard being led home by his more sober friends (Plate 41[3]). As a rule, however, the effigy vessels stick to the one-figure representation and abhor eccentric, wild movement. They are more or less dominated by the "static" principle which characterizes all true plastic works.

Quite another tendency is to be found in low relief and in the vase paintings, which can be considered together. They are dominated by the principle of vivid and even dramatic action and by the representation of multifigured groups. Wild and quick movements, such as fast running, throwing a spear, *etc.*, are depicted in a masterly way. Furthermore, the number of figures which are painted on a vase may run up to more than sixty. These scenes afford an excellent opportunity to study the interrelations, the actions, and functions of the various beings, human or mythical, which might appear more or less isolated in other representations. In these dynamic paintings, we have an excellent chance for a deeper insight by functional analysis.

To estimate the vase paintings in a right and proper way and to the fullest extent, a precondition must be fulfilled. It is not sufficient to have a photographic reproduction; it is also necessary to have an "extension" or an "unrolling" made. The photographic reproduction can give but a detail, and even this detail will have foreshortenings.

Some concrete examples should be considered here to give an idea of the abundant material for iconographic study. Furthermore, the should be mentioned, since the effigy vessels and vase paintings do not depict all aspects of life. This holds true for the primary source of existence: agriculture. Archaeological investigation leaves no doubt that the main subsistence was won by a highly developed agriculture with artificial irrigation. The remains of these irrigation systems are still to be found everywhere. Many vessels are in the shape of, or decorated with, representations of Indian corn, beans, squashes, potatoes, manioc, *etc.* Nevertheless, representations of agriculturists at work seem to be extremely rare, if they exist at all.

On the other hand, even in this highly developed civilization, hunting, fishing, and even the gathering of snails must have played an important role, as all of this has been depicted many times. Admittedly, the hunting of the deer was more a kind of sport indulged in by the noble warriers. Not only the deer, but also the fox, was hunted in the deserts between the valleys, indicated in the drawings by little, sandy hills and cactus plants (FIGURE 1).[3] The characteristic trait of the fox is the furry, half-dark, half-clear tail, by which he can be identified without any doubt. This same principle applies to all other animals, as with the art of the northwest coast of North America, in which animals are always characterized by special features.

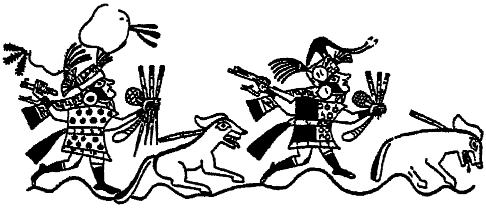

FIGURE 1.

The two hunters wear short shirts and skirts which are decorated with little round metal discs. This rich dress, as well as the beautiful headdresses with the long eagle feathers, proves that they belong to the nobility. For weapons, they have, besides the sling, light javelins, which are thrown with an "atlatl." One of the hunters is just throwing his spear, the other appears in the same position, although he has already dispatched his spear. This is a kind of erroneous representation which can also be found in the Maya codices.

Since the waters of the Peruvian coast teem with fish, fishing was extremely important from the beginning. The Chimu used the same light rafts, made of reeds, which are still in use today on the northern coast. These "balsas" appeared to the fantasy of the Chimu as a kind of fish on whose "back" they embarked on the open sea. Thus the "balsa" in FIGURE 2 takes the form of a big fish: prow and stern of the raft-like boat are decorated with fantastic animal heads, and big fins appear on both sides of the craft.[2] The reeds used in the construction are fastened together with thick ropes. The fisherman sits in a Buddha-like way, holding a long rope in his right hand and the broad, board-like paddle in his left. Wave bands on all four sides give a solid frame to the composition. In the right corner, two houses are to be seen, which give a certain idea of the simple type of dwellings in which the fishermen lived.

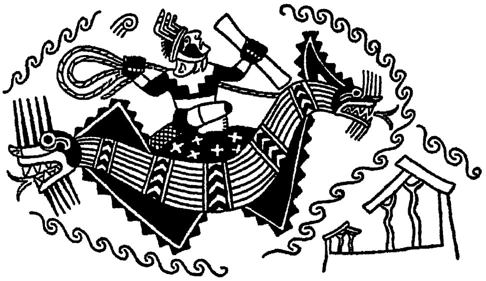

FIGURE 2.

A second big group of representations is devoted to the war-life. An effigy-vessel in the British Museum gives a good example of what the warriors of this time looked like (Plate 18[3]). Their equipment and weapons were rather standardized. The head is protected by the characteristic pointed helmet; under the short, armored shirt, which is decorated with spirals, the loin cloth is visible. The round shield is reinforced by small bands of copper, and the heavy war-club is furnished with a ring shaped knob. The vase paintings depict every phase of the war in an almost movie-strip like way. There are warriors on the march, approaching the enemy, and there are many battle scenes which are full of dramatic action. To this type belongs a scene which shows the flight of a warrior who is pursued by the victor (Figure 23[3]). The latter, protected by an armor of parallel staves, brandishes his bit war club. The fleeing enemy has already thrown away his club, armored shirt, helmet, and headdress. The large, bell-like ornament, which is the characteristic attribute of the warrior, starts to become unfastened from the belt.

An even better idea of the warriors dress and weapons is derived from another group of vase paintings which almost look like a kind of "Sears and Roebuck" catalogue. To the big war club are attached all the trophies taken from the enemy: a helmet, the characteristic bell-shaped ornament, *etc.* (FIGURE 3).

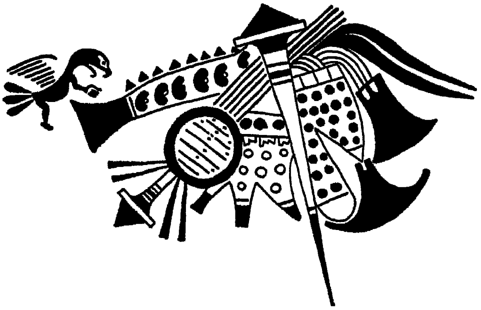

FIGURE 3.

After a battle, the victors would start for home, leading their naked captives by thick ropes tied around their necks. One of the large flaring rim bowls shows a column of this type. Three of the prisoners are trying in vain to stop the blood from pouring out of their noses, while the fourth turns back in an accusing gesture (Figure 25[3]). The warriors have shouldered the heavy war clubs, made heavier by the trophies attached to them. The column is headed by a kind of herald, whose belt ornament consists of the skin of a little fox-like animal. Humming birds surround the procession, which moves in quick tempo through the sandy hills, with their cactus and other desert plants.

What fate awaited the prisoners of war in the land of the victors is shown by a beautiful vase painting in the collection of the American Museum of Natural History in New York (FIGURE 4). Clark Wissler has quite correctly identified "the unexpected guests," as he calls them, as "officials with captives from 'the front'."[4] As a matter of fact, we see the chiefs of the defeated enemies carried in litters by their naked subjects. The bare head, as well as the strong rope around the third chief's neck, indicates that they are prisoners. The column approaches a great step-pyramid of the type so often encountered in these valleys. The platform of the pyramid is crowned by a richly decorated building. In the interior of this building, which might be the seat of a chief as well as a temple, a richly clad dignitary awaits the column, whose approach is announced by a smaller person, blowing a trumpet.

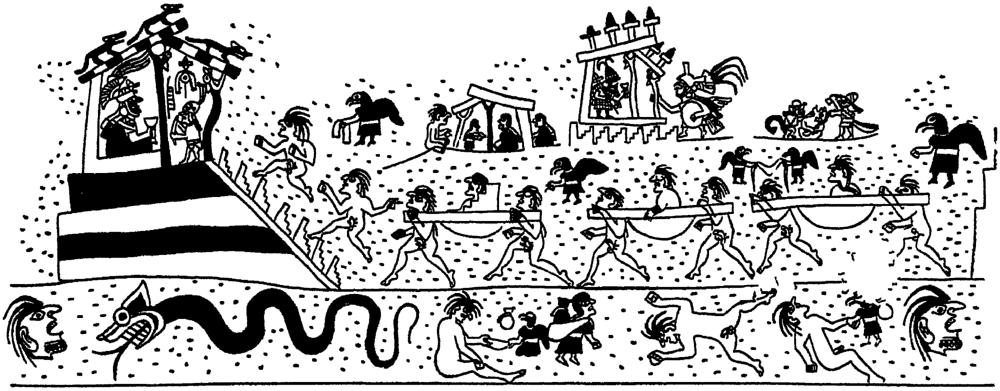

FIGURE 4.

Whereas this group has been known for a long time, having been published by several authors, the smaller accompanying groups have never been published, although they are of no less interest in the continuity of the story. There is, for instance, in the lower row, a naked man, lying flat on his back, his extremities extended. This is the characteristic position, in the battle-scenes, for indicating a corpse. In the same row, lies a severed human head. At least some of the prisoners, if not all of them, were sacrificed to the gods and demons, as is shown by a group in the upper row, above the litter bearers. There, we see two animal demons who are killing their victim with the typical sacrificial weapon: the tumi-like axe, which is never used in battle by the warriers but is reserved for ceremonies or for the gods in their fights.

In representations of this type, the close connection which existed between war and religious beliefs is indicated. As in Aztec Mexico, the main motif for the many wars seems to have been the winning of victims for human sacrifices, which were connected in some way with the "order of the world."

It is very interesting to note that the majority of these wars were of intertribal character. In general, both groups of warriors involved in a fight show the typical Chimu dress and weapons. This means that one group of the Chimu fought another, in the same way as among the ancient Greek city states. This intertribal fighting reflects the lack of a strong authority, dominating and uniting all the Chimu groups in the different valleys. The lack of political unification might also be the reason why the Early Chimu civilization did not spread as far to the north and south as did the late Chimu in its imperialistic phase.

In marked contrast to the primitive tribes, most of the higher civilizations of ancient America are noteworthy for a system of very severe punishments The Early Chimu are no exception to this rule The criminal was attached to a big pole and the skin of his face flayed to the point where it hung down like a mask. Then he was stoned to death and left to the birds of prey (Figure 29[3]). Seler suggested a long time ago that these representations might be connected with a very interesting astral myth reported by Antonio de la Calancha, telling about the severe punishment of thieves. Since a male and a female being are usually depicted, however, it could as well be punishment for adultery.

A decidedly more pleasant view of this civilization, in which the warriors played such an important role, is offered by the many dances and feasts. Here the warriors appear again in their full regalia, but without weapons. In addition to the typical helmet, the bell-shaped ornament, *etc.*, the dancers wore very tight gaiters with little bells or rattles affixed. The dancers always formed a long chain by holding the wrist of the next following man. The whole line was directed by two dancers who were especially richly dressed, one of them performing all steps backwards (Figure 31[3]).

Dances, music, and drinking bouts also filled the peaceful life after death. In one painting, the spirits form the characteristic chain, while two flutes and two pan-pipes furnish the music. Other death spirits stand before two enormous jars which contain the intoxicating beverage (Figure 33[3]). These representations of the dancing dead are rather unique in ancient American art and remind one in a surprising way of the medieval death dances of Western Europe.

Another group of vase paintings is devoted to a ceremony which must have been of special importance to this civilization. It seems to have been a kind of sacred race. The participants wore only their loin cloths and belts, but were adorned with beautiful head ornaments of special shape (Figure 36[3]) In almost every case, a very curious object is shown in one hand of the racers. It is a little bag, fashioned out of a piece of cloth or leather, which is folded in such a way that two corners stand up vertically above the fingers. These sacred races stand in close relation to beans, which formed one of the most important staple foods. This connection is shown quite clearly in a vase painting from the Museum in Bremen, where a number of beans follow the runners (Figure 38[3]). This is to express in a graphic way the idea that the beans form the content of the little bags.

The big races, which played such an important role in this civilization, were certainly not just a mere sport; they also had a magical and religious significance. They may be considered as a kind of fertility ceremony to insure the growth and rich harvest of the beans. They might be compared to quite analogous ceremonial races among the tribes of the southwestern U. S. There is also a very interesting old Spanish report from the hinterland of Lima, dealing with an analogous ceremony which clearly was meant to insure the fertility of a certain fruit.

Among other plants, coca deserves a special mention. The chewing of coca must have played an important role also in the ceremonial and religious life, as is shown by a vase painting in the Stuttgart collection (Figure 5[3]). The three seated figures hold the typical containers carried by coca chewers, from which they dip small quantities of lime to add to their coca cuds. The fourth figure, his face turned upward and his hands in an adoring attitude, has a coca bag and a lime container clearly depicted. Partly surrounding him is a double-headed monster, which stands in some relation to the heavens. As Doering has indicated, the Chimu, as well as the ancient Mexicans and Maya, symbolized the heavens as a double-headed monster

From representations of this kind we gain a certain insight into the rich ceremonial life which centered around the "huacas," the big temple pyramides. Other effigy vessels and vase paintings give us an idea what the demons and gods looked like.

The vast number of mythical beings, much larger than could be imagined from the report given by Antonio de la Calancha for the later period, can be split up, as has been shown elsewhere, into several groups, among which the animal demons outnumber all other groups. The typical attributes are the big tusks, a motif previously found in the Chavin civilization, and ear pendants in the form of serpent heads. Both attributes can clearly be seen in the beautiful crab demon, whose neck is encircled by a string of large pearls.

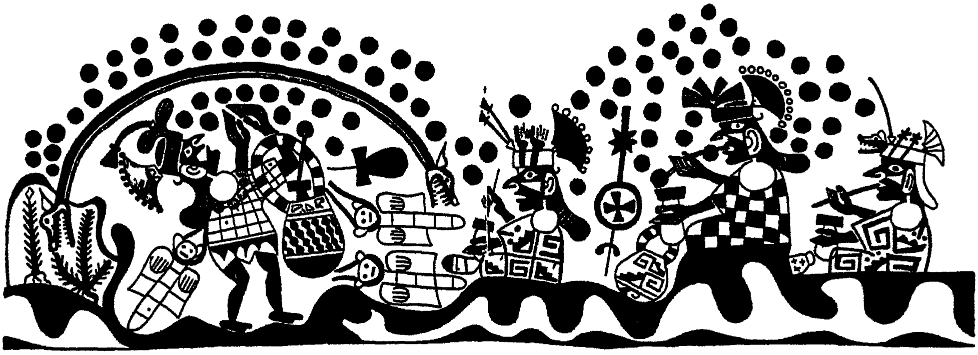

FIGURE 5.

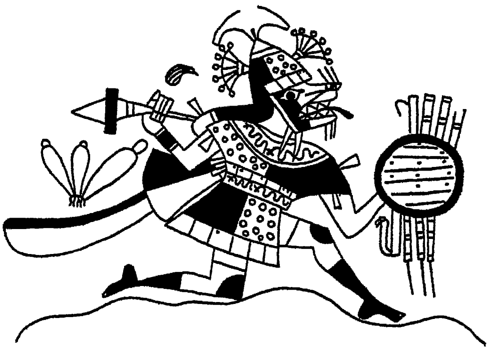

FIGURE 6

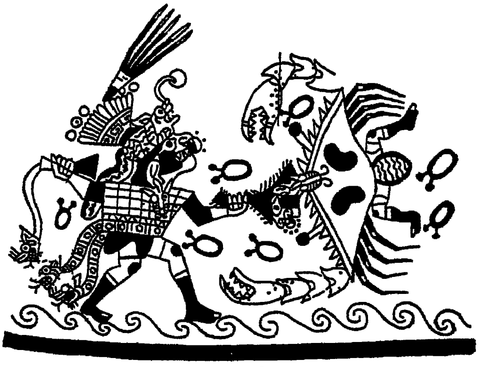

FIGURE 7.

The extremely war-like character of these animal demons is made clear by their dress and other attributes. They use the same weapons as the warriors of this time did, as can be seen in the fox demon from a vase in the American Museum of Natural History in New York (FIGURE 6). The demon uses helmet and armor, shield and club, and javelin and atlatl, whereas many other demons brandish a kind of "tumi," a very peculiar combination of axe and knife, also used in the human sacrifices. The same holds true for the plant demons, especially the bean demons, which are depicted as warriors wearing immense helmets In the middle of the bean, a human face is inserted.

How much the mythology reflected the real life of this period, which was crowded with wars, is shown by the many fights among the demons and gods. In these mythical battle scenes, there is a god of special importance, whose attribute consists of one or two snakes hanging down from his belt. This god, whose name is as unknown as those of his adversaries, is always victorious in the many fights in which he is engaged. His enemies are mostly crab and fish demons which he tries to subdue with his tumi or a long angler's line. A beautiful vase painting in the collection of the American Museum of Natural History in New York, published long ago by Squier, shows this god subduing a crab demon—grabbing him by the hair, the typical symbol of capture in war, in the Chimu civilization as well as in ancient Mexico (FIGURE 7).

Many scenes of fights among the gods, however, are not as simple as this. In a mythical battle scene from the Museum in Munich, there are more than sixty little beings represented (Figure 43[3]). Two groups of demons are involved in a fight: the animal demons fight against other demons which are personified objects, such as nose-ornaments, javelins, *etc.* The chief of the animal demons may be considered a god who is surrounded by an aureole, whose rays end in little snake heads. The battle ends with the victory of the animal demons, who bring the chief of the object demons as a prisoner of war to the deity with the aureole.

The same god is represented in another vase painting, accompanied by a number of animal and plant demons, his high rank being demonstrated by the fact that he is carried in a beautiful litter, while the two litter bearers hold, again, the little bags full of beans (Figure 37[3]).

We encounter this god with the aureole again in a vase painting which is divided into two zones by the horizontal body of the big serpent of the heavens. In the lower zone we see the litter of the god, surrounded by rays with little snake heads, and two demons, who are sacrificing two naked prisoners of war. On the back of the double-headed monster, a number of demons approach the god, who again is surrounded by many rays (FIGURE 8). This vase painting leaves no doubt that the god is a mythical being of supreme importance, whose dwelling is to be looked for in the heavenly regions. Doering has therefore not hesitated to identify this god with the moon god "Si," of whom Antonio de la Calancha has left us some information. This identification is supported further by a number of other representations, which show the god in a crescent or in a crescent-shaped "balsa" crossing the heavenly ocean (Figures 72 and 69[3]).

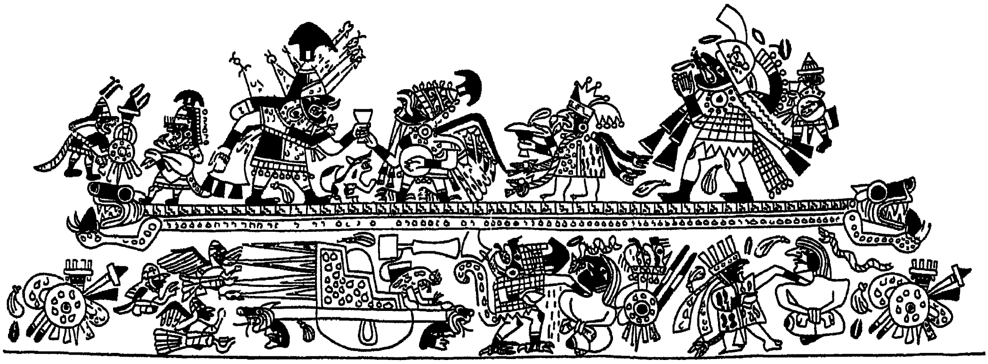
FIGURE 8.

Taken as a whole, the effigy vessels and the vase paintings of the Early Chimu may be compared to an enormous atlas of pictures of which the accompanying explanations are lost. No text gives us, as in the Greek vases or in the Egyptian tomb reliefs, the names of the gods or heroes represented. The deeper sense of these representations seems to be lost forever, except as their meaning may be recovered from the representations themselves. The key to an understanding, if there is any at all, lies in the meticulous combination and comparative analysis of the effigy vessels and the vase paintings.

If at some time it becomes possible to reconstruct partly the highly interesting civilization of the Early Chimu, and so to understand the basic principles of this culture, it will be thanks to those unknown and nameless artists who created these ceramics more than a thousand years ago. The monument which they have erected for themselves and for their people is not "*aere perennius*" but was created with the simple tools of potters, who were artists and, in some way, anthropologists at the same time.

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SECTION OF GEOLOGY AND MINERALOGY

THE PRECAMBRIAN-CAMBRIAN PROBLEM OF EAST NEWFOUNDLAND

By THOMAS N WALTHIER†

Newfoundland, the most eastern part of North America, is important in comparing the Paleozoic sediments of the New World with the type localities in the British Isles. The Cambrian and Ordovician are well exposed in many parts of Newfoundland and have been extensively studied.

The Structure of Newfoundland. In general, eastern and western Newfoundland are underlain by Cambro-Ordovician sediments folded along north-northeast trending axes. Folded Paleozoic strata also crop out along the coasts of Notre Dame and Bonavista Bays. The central and southern part of the island consists largely of granite with minor remants of folded Ordovician and Silurian sedimentary rocks. Throughout most of the island, the major folds are overturned to the west, and on the west coast they are arcuate in plan (FIGURE 1). The large, northern peninsula west of White Bay is anticlinal, exposing Precambrian crystalline rocks in the center, flanked by lower Paleozoic sediments. To the south, the Precambrian crystallines grade into schists and gneisses that are probably, in part, migmatized Cambrian sediments (Walthier, 1949: 14).

Several large thrusts have been recognized in southwest Newfoundland (FIGURE 1). The three western faults are known from field mapping (Cooper, 1936; Hayes & Johnson, 1938; Mook, 1926; Walthier, 1949, 1949a). However, the easternmost one is hypothetical. Its presence is inferred from high angle thrusts at White Bay (Heyl, 1937), and by the fact that Grand Lake lies on the strike projections in the granitic rocks to the south. This sixty-mile lake occupies a straight, narrow valley carved in the granites parallel to the faults at White Bay. Presumably, the valley represents differential erosion along a fault zone. The magnitude of the displacement shown on FIGURE 1 is conjectural.

Description of the Rocks of East Newfoundland. The basement complex of east and southeast Newfoundland is a thick sequence of acid lavas and tuffs, called the Harbour Main volcanics. It is composed of rhyolite, trachyte, and dacite, with minor amounts of andesite and devitrified acid glass. These rocks crop out over much of the area from the city of St. John's in the east to Fortune and Trinity Bays. At all the places studied, the lavas and pyroclastics are alike in appearance and stratigraphic position. Reconnaissance surveys suggest that similar type volcanics are continuously exposed between the mapped areas. For this reason, the Harbour Main volcanics are assumed to be everywhere of about the same age.

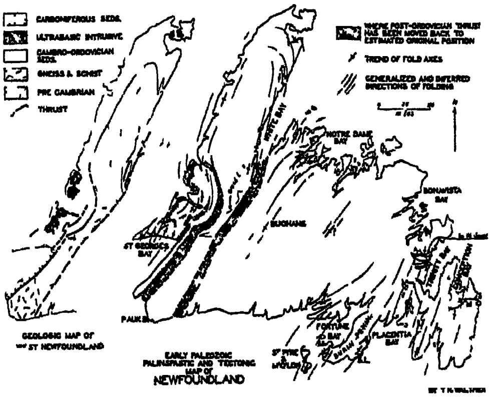

FIG 1

Overlying this volcanic material are sedimentary rocks separable into two large, complex divisions. The thickness of the older, unfossiliferous graywacke, sandstone, and shale division is highly variable and may be subdivided by one or two angular unconformities. The dominantly shaly younger series carries abundant Lower, Middle, and Upper Cambrian fossils. In a few places, the two divisions are conformable; usually, they are not. The lower may be entirely missing, while in many localities the fossiliferous beds are absent, presumably largely because of post-Ordovician erosion.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, April 3, 1950

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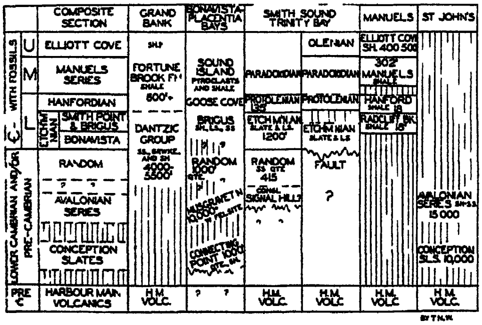
FIGURE 2 summarizes the stratigraphic sections of six localities, arranged from west to east, in east Newfoundland. The Harbour Main volcanics are assumed to be all of the same age. The fossiliferous Cambrian sediments have been correlated paleontologically. These sediments, particularly the highly fossiliferous Paradoxidian beds of Middle Cambrian age are particularly favorable for correlation and have been carefully studied at Manuels by Howell (1925). The unfossiliferous sediments above the volcanics cannot be traced from map area to map area because of their highly variable character. Consequently, they have been shown (FIGURE 2) in their relative stratigraphic positions only. Thus, the Musgravetown Group and the Avalonian Series are not necessarily synchronous, although they do have the same relative stratigraphic positions in their respective areas. So, too, with the other unfossiliferous sediments. Vertical shading represents absence of sediments.

FIG. 2
STRATIGRAPHIC SECTIONS
EASTERN NEWFOUNDLAND



At St. John's in the east (Hayes, 1931; Buddington, 1919), the Harbour Main volcanics are unconformably overlain by 10,000 feet of schistose sediments and lavas in the lower part and flint-like sediments, sandstones, graywackes, and tuffs in the upper part. Together, they comprise the Conception slates (It is possible that the lower beds of the Conception slates are interbedded with the upper Harbour Main volcanics.) The slates are complexly folded, truncated by erosion, and overlain unconformably by 15,000 feet of black slate, sandstone, graywacke, and conglomerate of the Avalonian Series. The Avalonian Series is also folded and is now preserved in a large syncline (see FIGURE 3).

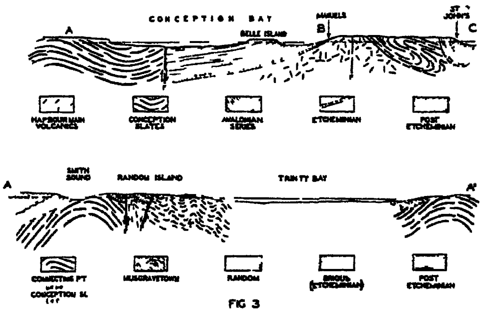


FIG 3
STRUCTURE SECTION
SMITH SOUND TO ST JOHN'S NEWFOUNDLAND

0 4 8
MILES
BY T.K.H.

Twelve miles due west of St. John's, at the fishing village of Manuels, fossiliferous brownish-black Cambrian shales lie unconformably on the Harbour Main rocks and a granite which is intrusive into the volcanics. These Cambrian beds have a well-developed basal conglomerate, with pebbles of the older volcanics and granite and a few limestone fragments.

More complicated relations are found fifty miles west at Smith Sound in Trinity Bay (Matthew, 1899; Walcott, 1900; Hayes & Rose, 1949). Below the highly fossiliferous Cambrian (*Protolenus* beds and younger) are shales and limestones carrying a meager, nontrilobite fauna. Matthew called similar sediments in New Brunswick "Etcheminian," and he correlated the beds in Smith Sound with those in New Brunswick (1899: 42–53). The Etcheminian at Smith Sound is locally conformable with the *Protolenus* beds. More often, a disconformity is recognized between them, and at one locality Matthew found the two separated by an angular unconformity (1899: 50). Conformably beneath the Etcheminian is the Random quartzite. The oldest exposed beds of the latter are conglomerates resembling the Signal Hill division of the Avalonian Series at St. John's (Walcott, 1900).

Hayes and Rose (1948) found the Random quartzite below the Brigus (Etcheminian) at Bonavista, Trinity, and Placentia Bays. Underlying the Random are 10,000 feet of conglomerate, volcanic rocks, sandstone, and shale, the upper part of which is like the Signal Hill formation. These latter sediments constitute the Musgravetown Group. Felsite lava forms a conspicuous part of the group. The contacts between the Musgravetown and Random and Random and Etcheminian are locally conformable, but in places unconformable. The geologic map of the area (Hayes and Rose, 1948, Plate 1) suggests slight overlap and offlap conditions in laterally shifting basins of deposition. The Connecting Point Group, the oldest rocks Hayes and Rose recognized in the area, consists of 1,000 feet of quartzites and shales. The Connecting Point and Musgravetown Groups are separated by an angular unconformity.

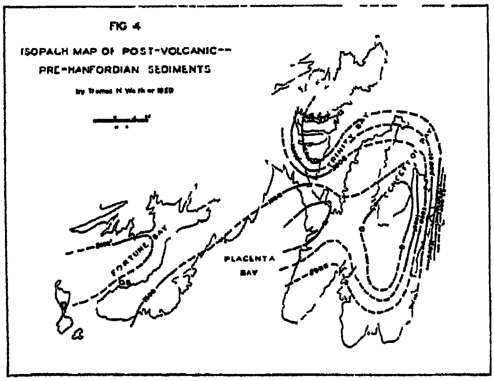


FIG 4

ISOPACH MAP OF POST-VOLCANIC— PRE-HANFORDIAN SEDIMENTS

At Grand Bank, in the southern part of the Burin Peninsula (Walthier, report in preparation), fossiliferous Cambrian shales of the Fortune Brook formation* conformably overlie 4,000 to 5,500 feet of essentially unfossiliferous graywackes and shales of the Dantzic Group.* The latter rest upon the truncated folded lavas of the volcanic basement. De La Rue (1932) found a similar stratigraphic section twelve miles to the west on the islands of St. Pierre and Miquelon.

Tectonic Behavior during Precambrian—Cambrian Time. Over short horizontal distances, the thickness of the sediments lying between the Hanfordian beds and the Harbour Main volcanics varies rapidly. FIGURE 4, an isopach map of these rocks, shows no trend in the contours. The picture is one of small, oval and rectangular areas with little or no sedimentary rocks of that age, but in adjacent localities many thousands of feet of these sediments are found, such as under Fortune Bay, at Smith Sound, and around St. John's. These thick sedimentary sections may contain at least one major angular unconformity separating the sediments into older and younger groups.

Such abrupt changes of thickness, particularly where a rectangular pattern is formed, implies that eastern Newfoundland was not a structural unit during this time and that small segments of the crust were dominantly positive, while adjoining ones sank several miles. Some may have sunk more or less uniformly. The block near St. John's, however, subsided some two miles and was then folded. Uplight and erosion followed and still later a renewal of subsidence amounting to 15,000 feet formed a basin in which the Avalonian sediments were deposited. Later, the area was again folded.

The only known limestone in Newfoundland which could have supplied the limestone pebbles to the basal conglomerate at Manuels are the thin beds of the Etcheminian Series. During Lower Etcheminian time, the area around Manuels was being eroded, while, in some adjacent sinking area, mud and lime were being deposited. In late Etcheminian or early Hanfordian time the relations were reversed. The previously sinking area was rising and supplying limestone detritus to the Manuels basin, which was just beginning a long period of submergence. Here, then, is definite evidence of two adjacent areas with opposite and reversing vertical movements.

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* New names.

A structure section from Smith Sound to St. John's (FIGURE 3; adapted largely from Hayes, 1931, & Hayes and Rose, 1948) illustrates this variability in thickness of sediments in adjacent areas. It also shows that the greatest stratigraphic changes along the line of section occur at major faults. For example, the 10,000 feet of the Musgravetown Group thins to zero along a fault. This thinning did not result from later erosion, for the Random quartzite overlies the Musgravetown to the east of the fault while it lies on the Connecting Point Group on the west side. Since the Random is locally conformable with the Musgravetown, 15,000 feet of the latter cannot have been eroded to the west of the fault between the deposition of the Musgravetown and the Random. It would seem that the Musgravetown was never deposited to the west, and the inequalities in thickness along the fault zone are primary. The Conception slates also thin to zero rapidly along faults on either side of Conception Bay.

The geology for southeastern and eastern Newfoundland from Harbour Main to Hanfordian time is summarized thus: (1) rapid lateral change in thickness of sediments outlining local areas that acted as tectonic units independently of adjacent areas; (2) active volcanism, represented by tuffs and lavas interbedded with many of the sediments; (3) clastic sediments, with graywackes abundant and limestones rare, indicating moderate to high crustal instability (Krynine, 1948: 153); (4) local angular unconformities; (5) reversing positive and negative movements; (6) many sharp lateral changes of sedimentary thickness along major faults presumed to have been active during sedimentation; (7) granite intrusion is post-Harbour Main and pre-Hanfordian time.

This type structural-stratigraphic pattern has all of the characteristics associated with the Tertiary history of the Coast Ranges of California (Clark, quoted by Bucher, 1933: 310). A region having such a complex structure has been termed a heterogeneous mobile belt by Bucher (1933: 267) and is much like an epieugeosyncline (Kay, 1947: 1293).

To be certain that the pre-Hanfordian rocks of eastern Newfoundland represent the same type of structural conditions as those of the Coast Ranges, it would be necessary to know that the Newfoundland faults were contemporaneous with sedimentation and that much of the rapid stratigraphic changes occurred along such dislocations. This cannot be done, yet the abrupt change of sedimentary thickness across the known faults is good, but indirect, evidence that the faults were active during sedimentation and limited the basins of deposition. On the other hand, the known faults might represent later displacement along what were very sharp flexures during the deposition of the unfossiliferous rocks. Even so, the structural pattern would still be much like that in Southern California during the Cenozoic.

Correlation of the Unfossiliferous Sediments. Since it now seems likely that the tectonic history of southeast and east Newfoundland during the Cambrian, and perhaps earlier times, was like that of the Coast Ranges, the existing correlations of the pre-Hanfordian sediments in Newfoundland should be reviewed.

Many previous workers have assumed that a uniformity exists within the strata throughout eastern Newfoundland and that angular unconformities are of wide extent and everywhere approximately synchronous. With this assumption, an unconformity ought to be correctly correlated by its relative stratigraphic position. Moreover, rocks of the same relative age in two areas should also be of about the same chronologic age.

From this account, it would appear certain that there is no uniformity and continuity to the sediments and unconformities of the pre-Hanfordian rocks in east Newfoundland. All evidence points to a unique history for each of the restricted areas. Consequently, rocks of the same relative age in two areas are not necessarily of the same chronologic age, nor is there any way to correlate these beds when only their relative age is known. When the intermediate areas have been mapped, it may be possible to trace some units across the different crustal blocks and establish a correlation.

The Precambrian-Lower Cambrian Contact. (a) *Nature of the Contact.* Precambrian rocks are usually defined as those separated by an unconformity from younger rocks carrying Cambrian (preferably Lower Cambrian) fossils. By such a definition, Precambrian rocks are extensively developed in east Newfoundland. Cambrian rocks are recognized by their fossil faunas. The boundary between Precambrian and Lower Cambrian rocks should be a time-stratigraphic boundary, that is, it should separate rocks of two ages, and, ideally, it should be synchronous throughout the world. To determine such a boundary requires precise correlation of the overlying and underlying rocks. Generally, the most effective methods in correlation are paleontological, and this is now the basis of all long-distance correlations of sedimentary beds. As Hedberg has pointed out, however, "Paleontologic features are at present the most useful means we have for extending time-stratigraphic surfaces, but even they are *always imperfect*" (1948: 455). Thus, even when dealing with fossiliferous sediments above and below a boundary, it is difficult, if not impossible, to establish the synchroneity of the boundary at all places. Unfossiliferous sediments are almost entirely unsuited for detailed time determinations. The lack of fossils in Precambrian sediments makes it impossible to establish that the Precambrian-Cambrian boundary is of exactly the same age in all parts of the world. In practice, it is usually placed at the first marked unconformity below fossiliferous Cambrian sediments. Thus, the base of the Paleozoic is fixed arbitrarily, although the character of the contact is often overlooked.

(b) *The Boundary in East Newfoundland.* In eastern Newfoundland, rocks carrying typical Lower Cambrian fossils are underlain, commonly unconformably, by unfossiliferous sediments (Precambrian by definition). The unconformities are of such local extent in east Newfoundland, however, that the boundary placed at an unconformity in one locality would be practically impossible to trace into other mapped areas. Also, rocks considered Precambrian because they are unconformable beneath fossiliferous Cambrian sediments may be conformable with the Lower Cambrian beds a few miles away, where they would be regarded as part of the Cambrian system. It is evident that no criteria are available to distinguish precisely between Lowest Cambrian and the uppermost Precambrian in east Newfoundland. Strict application of the definition of Precambrian rocks gives anomalous correlations.

Although the Precambrian-Cambrian contact is generally regarded as one of the major boundaries in the geologic time scale, its recognition in east Newfoundland is elusive indeed. East Newfoundland seems to be a locality where Precambrian rocks merge into Lower Cambrian ones. Paleontologically, too, they are gradational. The oldest sediments are entirely unfossiliferous. These grade up into the sparsely fossiliferous Lower Etcheminian and finally into the abundantly fossiliferous beds of the Lower and Middle Cambrian.

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*SECTION OF PSYCHOLOGY**

THE DENES ACTION METHOD OF PERSONALITY DEVELOPMENT†

By EMILY T. BURR‡

There are three objectives in the Denes Action Method. The first is to educate the public by presenting, through the dramatizations of actual case histories, some of the outstanding reactions of neurotic and maladjusted individuals whose roles are set in disturbing life situations. These case histories were dramatized by Gyula Denes, author and playwright from Budapest. The second objective is to indicate how this technique can be employed by clinical directors as a part of their regular program of group therapy. The final objective is to remove the stigma attached to mental disease by bringing to the public an awareness of the true nature of certain types of mental illness—to bring insight to the individual himself regarding his behavior and thus help him to alter his way of life. In this fashion, the public attitude toward mental difficulties may be so influenced that, whenever the seriousness of an individual's mental condition warrants it, early treatment and perhaps even the need for hospitalization will be recognized with no more opprobrium attached thereto than would be the case if he had an attack of heart disease or tuberculosis.

The Denes Action Method has two phases. (1) It presents dramatic incidents based on actual cases that are acted by professional actors and actresses and interpreted by a psychiatrist or psychologist. (2) It offers opportunities for persons in the audience to play roles in an unrehearsed, spontaneous presentation of personality problems in simulated life situations. Both phases of this action technique have proved to be highly successful in helping some persons to overcome their own personality difficulties and, by understanding better the difficulties of others, to remedy their unfortunate behavioral patterns. It is the opinion of many psychologists and psychiatrists that it is possible for a neurotic individual to achieve a definite degree of mental catharsis through witnessing such dramatic presentations as these, listening to the discussion of the theme, and perhaps participating in the discussion himself. The release of tension in the hitherto retiring individual who responds freely in a friendly atmosphere shows the efficacy of the method.

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* There was no meeting of the Section of Biology in April.

† The meeting at which this paper was presented, April 17, 1950, was the first held in the new building of the Academy.

‡ Guidance Bureau, Psychological and Vocational Service, New York City.

It is a truism that we evolve our standards and ideals out of our associations with others, but we cannot hope to arrive at maturity of judgment until we have acquired insight into the underlying motivations of these individuals and increased the depth of our own understanding of the principles of human behavior and the fundamentals of mental health. It is essential, in order to arrive at a recognition of the psychological factors inherent in each case, to note individual difference and to be ready with a happy, sensible response that will meet the needs of that person.

The psychological medium of dramatizing case histories is a form of entertainment that impresses an audience by its oral, pictorial, and emotional appeal. The human problems thus portrayed remain in the memory, as later interviews with persons who have witnessed the dramas have attested. The intensity of their interest shows how great a need there is for information regarding peculiarities of behavior and possible symptoms of mental disorder. There is a universal desire to know how to deal with personality disturbances in the home, in the schoolroom, and in business.

Dramatizing the human situations that arise in business is an effective method of demonstrating to clerks, sales persons, and executives how attitudes may be improved so as to bring about desirable employee-management and employee-customer relationships. This method has proved very successful. Such a form of educational procedure involves the use of the drama in business concerns as a specific method of training employees. Instead of the old-fashioned lecture procedure and all its "do's" and "don'ts," employees are given dramatic presentations of actual everyday office and store incidents enacted by attractive and experienced actors and actresses who put over their parts in truly professional style. Both desirable and undesirable methods of dealing with people are shown. Each play is followed by an explanation presented by an industrial psychologist familiar with business methods. Instead of being bored and restless, the employees listen attentively, and there are some who wish to discuss controversial points. Sometimes, different modes of behavior are put to vote, or an experienced employee may offer suggestions that will be acceptable to the group. This type of teaching is impersonal and gives the employee a chance to identify himself with the character whom he may most closely resemble or whom he may wish to resemble. Seeing his weak points, as well as his strong points, is enlightening. In nine cases out of ten, he will try to imitate the attractive actor's performance, for, in his heart, he longs to be "tops" in his job. Dramatizing his job glamorizes it. He is made to feel that doing his job well is rewarding.

Another way in which the shy, withdrawn, and introverted personality may become freed and responsive is through participating in the Denes extemporaneous drama, in which each person is given a part to play in the framework of a common situation. He must interpret his part as he sees fit. This gives him an opportunity for self-expression and for the development of spontaneity and initiative. It simulates a real life situation and demonstrates the need for alertness and quick thinking, since rarely does one have time to consider how to respond to unfamiliar demands. Such exercises as this extemporaneous drama offers help to develop the free expression of ideas, to dissipate fear, to increase self-confidence, and to encourage a *savoir faire* that often results in positive personality gains. Inasmuch as those who take part in these spontaneous productions must quickly organize their thoughts into pertinent remarks that will fit into the structural pattern established by the other participants, rapid and clear thinking is necessary. It means that co-operation of a high order is attained.

Children readily enter into an activity of this sort, and the method has proved eminently successful in dealing with young delinquents. In the Brooklyn Training School, the children ranging in age from 10 to 16 were asked to outline a play which they would enjoy acting. At once they suggested a case of truancy. Another time, they suggested the case of a girl unjustly accused of stealing a blouse. (When the owner of the blouse discovered that she had made a false accusation, she apologised for her mistake.) In all cases proposed by members of the delinquent group, a chance for free discussion under expert leadership was permitted. The right action was commended, and the children themselves volunteered to point out different ways of handling the situation. Getting the children to think out their own problems seemed to be a salutary method. The variations in behavior brought out a broad aspect of behavioral techniques. This educational method set up a dynamic pattern that was wholly practical.

The Denes Action Method of Group Therapy has many applications. It is not a substitute for psychoanalysis, which is too expensive for the individual of moderate means, but it has been extremely beneficial in cases of individuals showing minor mental and emotional disturbances. The theory back of the Denes Action Method is to prevent the development of unhealthy mental attitudes and to help individuals to acquire insight into the underlying causes of their difficulties so that a satisfactory personal adjustment may be made. In this way, it may become possible for them to see through the dark circumstances that apparently surround them and to achieve the personal sense of power that accompanies the capacity to control the self and so succeed in making a happy life-adjustment.

SECTION OF PHYSICS AND CHEMISTRY

SOME RECENT MEASUREMENTS OF THE CONDUCTANCE OF DILUTE, AQUEOUS ELECTROLYTE SOLUTIONS[*]

By CHARLES A. KRAUS[†]

The results here presented have been accumulated over a period of several years by a number of different workers in the Metcalf Research Laboratory of Brown University. The chief objective of these investigations was to determine the effect of the constitution and structure of ions on their conductance in water. The ions investigated include the symmetrical quaternary ammonium ions from tetramethyl- to tetra-normal-amylammonium, inclusive. Another series includes the unsymmetrical ions from ethyltrimethyl- to octadecyltrimethylammonium, inclusive, in steps of two carbon atoms in the long chain.

Since the salts of unsymmetrical long chain ions are colloidal electrolytes in water, with critical concentrations lying as low as $1 \times 10^{-4} N$ in some instances, it appeared desirable to improve the techniques employed in making up these solutions and in measuring their conductance.

Experimental Techniques. Measurements were carried out with solutions in a Pyrex cell of the Erlenmeyer type that had a capacity of 850 cc. of liquid. Salts were weighed out in quartz cups on a microbalance.[1] Manipulations with the cell, such as introducting water and salt, were carried out in a rubber-gloved chamber, within which the atmosphere was conditioned to have a constant content of carbon dioxide. This atmosphere was obtained by passing air through aspirator pumps and then through the gloved chamber.[2] Water in equilibrium with this atmosphere had a specific conductance of 3–5 × 10^{-7} mho and was very constant. The specific conductance of the water in the cell during manipulation remained constant within a few units in the tenth place of decimals.

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[*] This paper, illustrated with lantern slides, was presented at the meeting of the Section, April 18,1950.

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Test of Experimental Method. To test the reliability of the apparatus and procedure employed in the measurements, the conductance of solutions of potassium chloride was measured at concentrations between 1 × 10^{-4} and 1 × $10^{-3}N$.[1] The results are shown in the first two lines of TABLE 1. The experimental values are reproduced by the equation:

$$\Lambda = 150.02 - 96.0\sqrt{C} \qquad (1)$$

with an average deviation of less than 0.01%.

TABLE 1

COMPARISON OF OBSERVED AND CALCULATED VALUES OF Λ FOR KCl IN WATER AT 25°*

$C \times 10^4$	1.1556	2.6924	5.2463	8.9078	10.092
Λ obs	148.97	148.44	147.83	147.15	146.98
Λ calc	148 97	148.43	147.81	147.16	146.98
$\Delta\Lambda$	0.00	+0.01	+0.02	−0.01	0 00
$C \times 10^4$	3.3448	5.0999	7.3643	8.9915	
Λ obs.	148.27	147.84	147.37	147.13	
Λ calc	148.25	147.84	147.41	147.14	
$\Delta\Lambda$	+0.02	0.00	−0.04	−0 01	

* $\Lambda = 149.98 - 94.535\sqrt{C}.$

If we assume Onsager's equation to hold and assume the specific conductance of a 1 × $10^{-3}N$ solution of potassium chloride to be 146.98 × 10^{-6} and substitute in Onsager's equation, we obtain Λ_0 = 149.98. The equation becomes:

$$\Lambda_0 = 149.98 - 94.535\sqrt{C}. \qquad (2)$$

Conductance values computed according to equation (2) are shown in the third line of TABLE 1 and deviations $\Delta\Lambda$ in the last line. The average deviation is 0.012 Λ-unit, or less than 0.01 per cent.

The conductance of a considerable number of other salts has been measured, although with somewhat less care than was observed with KCl. In TABLE 2 are shown Λ_0 values computed according to the Onsager equation (column 2) and extrapolated values (column 3). Differences between the two are shown in the last column. The average difference is 0.03 Λ-unit, and positive and negative differences are nearly equal.

TABLE 2

EXTRAPOLATED AND CALCULATED Λ_0 VALUES (ONSAGER'S EQUATION)

Salt	Λ_0 (*Calc.*)	Λ_0 (*Extrap.*)	*Diff.* Λ_0
$KBrO_3$	129.24 ± 0.04	129.28	−0.04
Me_4NPi	75.31 ± 0.01	75.35	−0.04
Et_4NPi	63.05 ± 0.02	63.01	+0.04
Pr_4NI	100.25 ± 0.03	100.26	−0.01
Bu_4NBr	97.61 ± 0.02	97.57	+0.04
Am_4NBr	95.61 ± 0.01	95.62	−0.01
KBr	152.05 ± 0.01	—	—
KPi	103.96 ± 0.02	—	—

So far as present results are concerned, Onsager's equation holds within the limit of experimental error at concentrations below 1 × $10^{-3}N$ for all the salts measured in these investigations. However, Shedlovsky[3] has found that, for KCl and some other salts, conductance values may be reproduced within the limits of experimental error up to a concentration of 0.01N, or higher, by means of the equation:

$$\Lambda = \Lambda_0 - \alpha\sqrt{C} + (\alpha^2/\Lambda_0^2)C\Lambda. \qquad (3)$$

If we compute Λ_0 for KCl on the basis of equation (3) and a specific conductance of 146.98 × 10^{-6} at a concentration of $10^{-3}N$, we obtain Λ_0 = 149.93, a value 0.05 Λ-unit lower than that computed according to equation (2). While the difference between the two equations is small, it would seem that further careful measurements might resolve the question as to which equation is applicable in the concentration range of 1×10^{-4} to $1 \times 10^{-3}N$.

Conductance of Symmetrical Quaternary Ammonium Ions. Using the Λ_0 values of TABLE 2, the value 149.98 for the limiting conductance of KCl, and 0.4906 for the transference number[4] of the potassium ion in KCl, we obtain the ion conductances of TABLE 3. Values for the iodide and nitrate ions are derived from the conductance data of Shedlovsky.

It is of interest to note that the picrate ion, with 16 atoms other than hydrogen, is but a little poorer conductor than the tetraethylammonium ion with 9 atoms and a much better conductor than the tetrapropylammonium ion with 13 atoms.

We are particularly interested in the conductance of the quaternary ammonium ions. In order to study the effect of added carbon atoms on conductance, however, it is preferable to compare ion resistances rather than ion conductances, as Thompson[5] has suggested. Since we shall want to compare ion resistances in water with those of the same ions in other solvents, we shall compare values of the reciprocals of the conductance viscosity product, $1/\Lambda_0^+\eta$.

In FIGURE 1 are shown values of $1/\Lambda_0^+\eta$ for the five quaternary ammonium ions from tetramethyl- to tetra-*n*-amylammonium, inclusive, as well as for the ammonium ion. In the same figure are shown resistance curves for the same ions in ethylene chloride, nitrobenzene, and pyridine.[6]

It will be noted that, with increasing number of carbon atoms in an ion, its resistance in water increases much more than it does in other solvents. In water, the smaller ions have a much lower resistance than in other solvents; the larger ions have a much larger resistance. It is of interest to note that the resistance change on replacing four hydrogen atoms by four methyl groups differs little from that which results on adding four methylene groups to a large ion.

Long Chain Ions. It is of interest to study the effect of chain length in the case of ions such as the alkyltrimethylammonium ions. If the alkyl chain of an ion contains eight or more carbon atoms, solutions of its salts exhibit colloidal properties at concentrations above what is known as the critical concentration. It is commonly assumed that, at concentrations above the critical, the long chain ions form aggregates or "micelles," as they are called. At concentrations below the critical, salts of long chain ions have been looked upon as normal electrolytes. We have investigated the behavior of these salts at low concentrations and have found that salts of ions having chains of sixteen or more carbon atoms are not normal electrolytes.[7, 8]

TABLE 3

ION CONDUCTANCES BASED ON Λ_0 for $K^+ = 73.58$

Ion	Λ_{0i}	*Ion*	Λ_{0i}
Cl^-	76.40	K^+	73.58
Br^-	78.47	Me_4N^+	44.93
I^-	76.80	Et_4N^+	32.67
NO_3^-	71.38	Pr_4N^+	23.45
BrO_3^-	55.66	Bu_4N^+	19.14
Pi^-	30.38	Am_4N^+	17.14

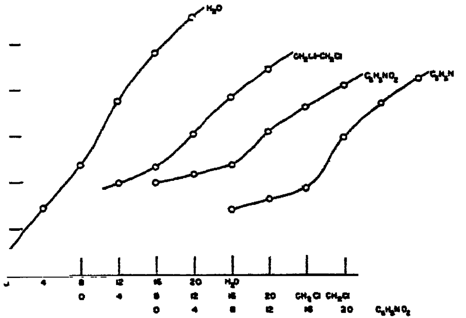

FIGURE 1. Resistance curves of quarternary ammonium ions (values of $1/\Lambda_0^+\eta$).

A. *Slope of Λ-$\sqrt{C}$ Plots.* In the case of normal electrolytes, the slopes of the Λ-$\sqrt{C}$ plots conform to Onsager's equation. With chain lengths of less than 16 carbon atoms, the slopes are normal; with 16 carbon atoms or more, the observed slopes are greater than the theoretical. In TABLE 4 are presented limiting conductances for a number of 1–1 salts of long chain ions. In the third column are shown the observed slopes, in the fourth, the theoretical slopes for 1–1 salts, and, in the last column, the ratio of observed to theoretical slope. For the last salt in the table, the slopes are also compared, assuming a 2–1 electrolyte.

TABLE 4

SLOPES OF LONG CHAIN SALTS

Salt	Λ_0	*Slope Obs.*	*Slope Theor.*	*Ratio*
$C_{12}H_{25}(CH_3)_3N\ NO_3$	93.86	81.7	81.7	1.00
$C_{14}H_{29}(CH_3)_3N\ NO_3$	92.50	82.2	82.2	1.00
$C_{16}H_{33}(CH_3)_3N\ NO_3$	92.20	142	81.3	1.75
$C_{18}H_{37}(CH_3)_3N\ NO_3$	91.34	143	81.1	1.76
$C_{16}H_{33}(C_4H_9)_3N\ BrO_3$	72.38	199.4	76.8	2.59
			142.0[b]	1.40

[b] For 2-1 Salt.

TABLE 5

CONDUCTANCE OF LONG CHAIN IONS

Ion	Λ_0^+	*Ion*	Λ_0^+
$C_2H_5(CH_3)_3N^+$	40.51	$C_{12}H_{25}(CH_3)_3N^+$	22.39
$C_4H_9(CH_3)_3N^+$	33.26	$C_{14}H_{29}(CH_3)_3N^+$	21.16
$C_6H_{13}(CH_3)_3N^+$	29.23	$C_{16}H_{33}(CH_3)_3N^+$	20.82
$C_8H_{17}(CH_3)_3N^+$	26.21	$C_{18}H_{37}(CH_3)_3N^+$	19.96
$C_{10}H_{21}(CH_3)_3N^+$	24.00	$C_{16}H_{33}(C_4H_9)_3N^+$	16.72

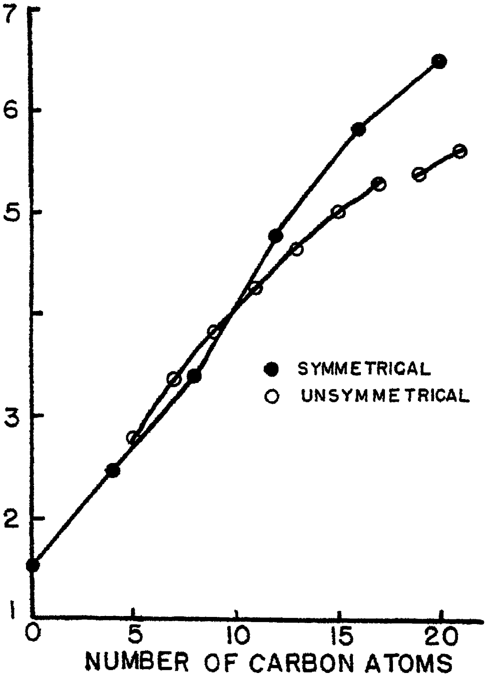

FIGURE 2. Resistance curves of symmetrical and of alkyltrimethylammonium ions (values of $1/\Lambda_0^+\eta$)

For ions having chains of 16 or more carbon atoms, the slopes for some salts approximate those of 2–1 salts. For the last salt in the table, the slope is markedly greater than that of a 2–1 salt. It is of interest to note that the observed slope is markedly dependent on the size of the three substituent alkyl groups of the long chain ions.

The steep slopes of the Λ-$\sqrt{C}$ plots might be accounted for by assuming ion-pair formation. Such interaction seems doubtful, however, in view of the fact that the corresponding dodecyl and tetradecyl derivatives exhibit a normal behavior. It seems more probable that, when chains reach a length of 16 or more carbon atoms, two (or perhaps more) chains associate, thus simulating an electrolyte of higher type.

B. *Ion Conductances.* In TABLE 5 are presented conductance values for alkyltrimethylammonium ions from ethyltrimethyl- to octadecyltrimethylammonium in steps of two carbon atoms per chain.[7, 8] The conductance of the hexadecyltributylammonium ion is also given. The effect of chain length on ion conductances will be better understood by reference to FIGURE 2, in which equivalent resistances ($1/\Lambda_0^+\eta$) are plotted as a function of the total number of carbon atoms in the ion. Similar values are also shown for the symmetrical quaternary ammonium ions.

It will be observed that the resistances of the ethyltrimethyl-, butyltrimethyl-, and hexyltrimethylammonium ions are somewhat larger than those of corresponding symmetrical ions. The two curves cross at a point that corresponds approximately to a heptyl derivative of a total of 10 carbon atoms. Beginning with the octyl derivative, the resistance increase with increasing number of carbon atoms is much smaller than it is for corresponding symmetrical ions.

There is a break in the resistance curve between the tetradecyl and the hexadecyl derivative. Such a break was not unexpected, in view of the fact that the ions of longer chains appear to be associated and the equivalent resistance of a multiply charged ion should be less than that of corresponding singly charged ions.

Conductances at High Field Strengths. As Wien has shown, when conductances are measured at higher fields, higher conductances are obtained, the magnitude of this effect being dependent on the nature of the electrolyte. For normal 1–1 salts, the limiting effect at very high fields corresponds to the conductance changes at lower potentials in going from a given concentration to the limit approached as the concentration decreases without limit. In practice, the resistance of a given electrolyte is balanced against that of potassium chloride at the same concentration. With normal 1–1 salts, the resistance change is the same as that of potassium chloride. With 2–1 salts, or higher, the resistance change is markedly greater than that of potassium chloride. It seemed that measurements of this kind with long chain salts might throw light on the results obtained by conductance measurements at low fields.

In FIGURE 3, the conductances of tetradecyltrimethylammonium salts are compared with that of potassium chloride up to field strengths of approximately 100 kilovolts per centimeter[9]. It will be noted that the conductances of the three salts are the same at different fields within the limit of experimental error.

In FIGURE 4 are shown conductance values for hexadecyltrimethylammonium nitrate and magnesium chloride[9] as a function of field strength at a concentration of $4 \times 10^{-4}N$. As may be seen from the figure, the two salts show the same conductance increase with field strength. At a field of 100 K.V./cm., the conductance increase is approximately 1 per cent. We may conclude that salts of ions having a chain length of 16 or more carbon atoms behave like electrolytes of higher type.

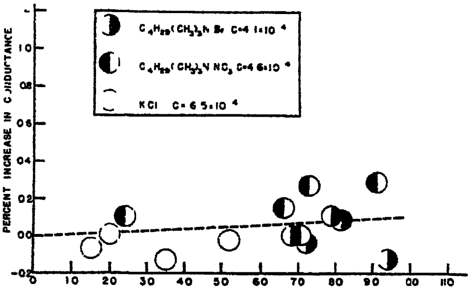

FIGURE 3. Conductance of tetradecyltrimethylammonium salts as a function of field strength

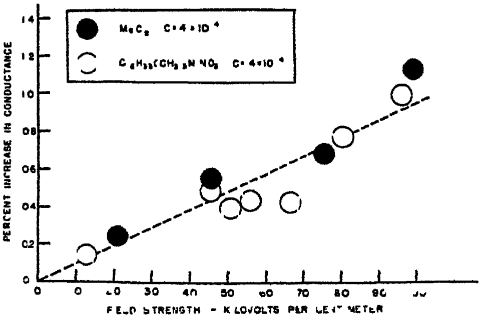

FIGURE 4. Conductance of hexadecyltriphenylammonium nitrate as a function of field strength

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SECTION OF ANTHROPOLOGY

VERO AND MELBOURNE MAN: A CULTURAL AND CHRONOLOGICAL INTERPRETATION*

By IRVING ROUSE†

In 1915 and 1916, human remains were found at Vero on the east coast of Florida in apparent association with the bones of Pleistocene animals (Sellards and others, 1917). Other such associations were obtained at Melbourne, 33 miles to the north, in 1923–29 (Gidley, 1929). These finds have aroused considerable controversy. Most of the paleontologists interested in them have assumed that the human remains were laid down at the same time as the Pleistocene animal bones, while most of the anthropologists have argued, on the contrary, that the human remains are intrusive into the Pleistocene strata and were deposited during a more recent period. Since both parties to the controversy have been able to cite data in favor of their viewpoint, the problem has never been satisfactorily resolved.

The writer recently had occasion to make a survey of the archeology of the Indian River area, in which the Vero and Melbourne sites are situated (Rouse, n.d.). While this survey was not directed specifically towards the problem of Vero and Melbourne Man, its results seem to have some bearing upon the problem, and the present paper is intended to discuss them from this standpoint.

First, it will perhaps be well to review the facts and theories concerning Vero and Melbourne Man. The three most recent geological formations in the Indian River area are, successively, the Anastasia, Melbourne, and Van Valkenburg (Cooke, 1945: 265 ff.). The first consists primarily of sand mixed with marine shells and was apparently laid down at a time when this part of Florida was under the ocean. The second, or Melbourne formation (also called Pamlico), is a land deposit, consisting of sand and a remarkably rich assemblage of animal bones, many of which represent species which became extinct at or after the close of the Pleistocene epoch. The Van Valkenburg formation, finally, is composed of muck and the bones of animal species still existing in the region today. (The few bones of extinct animals found in it are believed to be intrusive; Sellards, 1947: 964.)

Both the Anastasia and Melbourne and the Melbourne and Van Valkenburg strata are separated by unconformities. It follows that five intervals of geologic time are represented in the area: (1) an Anastasia interval, in which the sea was high enough to cover this part of Florida; (2) an interval which we shall call Anastasia-Melbourne, in which the sea had fallen to such an extent that the area became dry and was subject to erosion, causing the first unconformity; (3) a Melbourne interval, in which the sea again rose, but not high enough to cause any great amount of inundation; (4) a Melbourne-Van Valkenburg interval, in which a second lowering of the sea, accompanied by a fall of the water table in the interior, again exposed the land to erosion and caused the second unconformity; and (5) a Van Valkenburg interval, in which the sea and the water table rose a third time, producing the swampy conditions which prevail in the area today—or did prevail until the construction of drainage canals (Rouse, n.d.).

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* This paper, illustrated with Kodachrome slides, was presented at the meeting of the Section, April 24, 1950. The research upon which this paper is based was undertaken in connection with the Caribbean Anthropological Program of Yale University by means of a grant from the Viking Fund, Inc.

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No human remains are associated with either the Anastasia or the Anastasia-Melbourne interval. Both artifacts and human skeletal material have been found in the Melbourne deposits. They include several stemmed flint projectile points, flint chips, bone awls, and bone pins; a human cranium at the Melbourne site; and a few miscellaneous human bones at Vero. The last appear to form part of a much larger group of bones, including the cranium of Vero Man, which was found on top of the plane of contact between the Melbourne and Van Valkenburg formations and can therefore be correlated with the Melbourne-Van Valkenburg interval. Finally, potsherds, additional flint points and chips, bone awls, and bone pins; some other bone implements; and further human bones were obtained from the uppermost, Van Valkenburg layer (Sellards, 1937).

It is the finding of artifacts and human bones in the Melbourne formation, in apparent association with the bones of Pleistocene animals, which has aroused controversy. Three hypotheses have been advanced to explain this association. First, there is the possibility that man settled Florida during the Melbourne period and deposited his remains at the same time as the animal bones; in fact, he may have caused the extinction of the animals. Secondly, it has been postulated that man, first appearing on the scene during a later period, deposited his remains on the contact plane or in the Van Valkenburg formation and that these remains subsequently became mixed with the Melbourne material, possibly, as Chamberlin (1917) has suggested in the case of the Vero site, by a churning action of the streams which run through the sites. Finally, it has been argued that the remains represent burials made in modern times by Indians who dug through the Van Valkenburg layer, the Melbourne-Van Valkenburg unconformity, and into the Melbourne formation.

The third explanation was advanced by Hrdlička (1917) and, so far as the writer is aware, only by him. It is not in accord with the facts and can therefore be disregarded. The problem, then, is whether the human remains were deposited in the Melbourne bone bed at the time of its formation or became mixed in it at a later date by some such means as that suggested by Chamberlin.

The principal difficulty in the way of solving this problem is that, with one possible exception, the human remains are secondarily deposited. As already stated, they do not represent burials. Neither can they be considered refuse. Instead, they are derived from burial or refuse sites which are no longer in existence. If we knew the relation of these sites to the Melbourne or Van Valkenburg formations, we could solve the problem; in the absence of this knowledge, we cannot.

Let us turn, then, to the evidence provided by the primary sites still extant in the area, of which 202 are known as the result of our recent survey. We shall first consider these from an archeological rather than a geologic standpoint, *i.e.*, in terms of a sequence of cultural periods. Six of these periods are now known for the Indian River area: Preceramic, Orange, Malabar I, Malabar II, St. Augustine, and Seminole. Since the last two are entirely historic, they can, for our purposes, be disregarded. The others may be briefly characterized as follows.

In the Preceramic period, the principal artifacts were stemmed flint projectile points, possibly stone vessels, bone awls and pins, socketed antler projectile points, and shell gouges. In general, these fall within the range of the Preceramic horizons in other parts of the Southeast, *e.g.*, the Mt. Taylor period on the lower St. Johns River (Goggin, MS) and the Lauderdale period in the Tennessee Basin (Webb and DeJarnette, 1942), for which a date immediately preceding 500 A.D. has been estimated.

The Orange period is distinguished by the addition to the Preceramic complex of fiber-tempered clay pottery of the Orange Plain and Incised types (Griffin, 1945). Several new artifact forms, such as shell dippers and celts, also make their appearance. This period is related to a number of others throughout the Southeast which have been termed "Archaic" and are dated between 500 and 750 A.D.

During the Malabar I period, the fiber-tempered pottery develops into chalky ware of the St. Johns Plain, St. Johns Incised, and Dunns Creek Red types (Rouse, n.d.). (There are some grounds for subdividing this period into Malabar I and I′ on the basis of the presence and absence of St. Johns Incised pottery, but this need not concern us here.) So far as is known, all other forms of artifacts survive and a few new ones are added, notably the stone celt and shell pick. Burial mounds are absent or, at the most, very rare, but the period is definitely related to the Burial Mound horizons elsewhere in the Southeast and therefore may be dated between 750 and 1150 A.D.

The Malabar II period, finally, is marked by the disappearance of St. Johns Incised pottery and the rise to predominance of St. Johns Check Stamped. Burial mounds are definitely present. Stone vessels go out of use, but stone celts, bone awls and pins, socketed antler projectile points, and shell celts, gouges, picks, and vessels continue from the previous period. A series of new types, such as grooved stone pendants and engraved shell gorgets, make their first appearance.

It is clear that the Malabar II period, beginning about 1150 A.D., survived the coming of the Spaniards. Indeed, far from degenerating in the Colonial period, the native cultural complex was enriched by the addition of trade objects and of ornaments made of gold and silver looted from the wrecks of Spanish treasure ships. This, however, is another story.

From the standpoint of Vero and Melbourne man, it is important to note that the types of artifacts found in the Melbourne bed—projectile points, awls, and pins—are present throughout the cultural sequence, beginning in the Preceramic period and continuing well into historic times. They are of no use, therefore, as time markers. The absence of pottery in the Melbourne bone bed (with one possible exception) does suggest the Preceramic period, but this is not too reliable a point, since the deposit is secondary.

If we attempt to make the correlation on the basis of physical type rather than culture, we obtain a similar result. As Stewart (1946) has pointed out, Vero and Melbourne Man are physically of an earlier type than that represented in the burial mounds, *i.e.*, during Malabar II and possibly also the Malabar I period. We have no knowledge of the local physical type during the Preceramic and Orange periods, but elsewhere in the Southeast it corresponds to that of Vero and Melbourne Man. Therefore, on physical as well as cultural grounds, Vero and Melbourne man may be provisionally correlated with the Preceramic period, or possibly with the succeeding Orange period.

Let us next examine the relationship between the cultural and geologic periods. Evidence for doing this is found at several sites on the upper St. Johns River, notably Palmer-Taylor and South Indian Field. At these sites, as at Vero and Melbourne, three geologic formations are represented: (1) an underlying layer of marine shells, apparently the Anastasia of the coast; (2) the so-called Plummer loamy sand, which seems to correspond to the Melbourne bone bed (in fact, Melbourne fossils are reported to have been found in it); and (3) a top layer of muck, which is apparently the same as the Van Valkenburg formation on the coast (Rouse, MS).

At the Palmer-Taylor site, Preceramic refuse rests directly on the Plummer sand, whereas Malabar II material is found in association with the muck. At South Indian Field, Orange refuse lies on the sand, while some Malabar II specimens are included in the muck. There are similar indications at other sites that both the water table and the sea level were lower during the Preceramic and Orange periods than in the later periods (with the possible exception of Malabar I: Rouse, n.d.). We may therefore correlate the first two cultural peroids with the Melbourne-Van Valkenburg interval and the last three with the Van Valkenburg interval, as in TABLE 1.

Further evidence in favor of these correlations is supplied by the presence at both South Indian Field and the Melbourne Golf Course site of a series of small basins which, in the case of South Indian Field, appear to have been dug by Indians of the Orange period. At South Indian Field, these basins underlie the muck and extend down through the sand into the Anastasia formation. As a result, Orange and Malabar refuse, which has apparently washed into them since their abandonment, lies in hollows in the Anastasia stratum, well below the level of the Melbourne formation (Rouse, n.d.). At Melbourne, the basins are likewise on the surface of the Melbourne formation, underlying the muck, and can therefore be directly associated with the Melbourne-Van Valkenburg period (Gidley, 1929). For lack of a better hypothesis, we believe that they represent wells dug by the Indians during the Melbourne-Van Valkenburg period of low water.

Thirdly, it should be noted that no bones of extinct animals have been found in any of the Preceramic and Orange sites. There are two exceptions, both horse-teeth obtained from Orange refuse at South Indian Field, but these appear to have been picked up as fossils by the Indians (Rouse, n.d.). Thus, all the Indian occupations must be considered later than the Melbourne interval.

Correlation of the Preceramic and Orange cultural periods with the Melbourne-Van Valkenburg interval provides an indirect means of dating Vero and Melbourne Man. If, as we have indicated, Vero and Melbourne Man are Preceramic or possibly of the Orange period, it follows that they must have been contemporaneous with the Melbourne-Van Valkenburg interval. In other words, they are to be equated with the unconformity between the Melbourne and Van Valkenburg formations.

We are thus led to the conclusion that the Vero skull, which was found on the contact plane between the Melbourne and Van Valkenburg formations, represents the true state of deposition, while the Melbourne skull and the other material found in the Melbourne stratum at both Vero and Melbourne are intrusive. We suggest that Vero and Melbourne Man lived on the contact plane during the Melbourne-Van Valkenburg interval and introduced both bones and artifacts into the underlying Melbourne formation by making burials and digging wells, into which some artifacts and bones may subsequently have fallen, as they did at South Indian Field.

TABLE 1

CORRELATION OF THE GEOLOGIC INTERVALS AND CULTURAL PERIODS IN THE INDIAN RIVER AREA

Geologic intervals	*Cultural periods*
Van Valkenburg	Seminole
	St Augustine
	Malabar II
	Malabar I
Melbourne-Van Valkenburg	Orange
	Preceramic
Melbourne	
Anastasia-Melbourne	—
Anastasia	

A final word concerning the age of the Melbourne-Van Valkenburg period. It is assumed by most geologists that the height of the east coast of Florida has not changed since the middle of the Pleistocene and that the rise in sea level which took place at the close of the Melbourne-Van Valkenburg period was therefore entirely eustatic (*e.g.*, Cooke, 1945: 245). If this is so, it reflects the last major world-wide change in the level of the sea. According to Godwin (1940: 287, Figure 31), this took place in southern England, where conditions are also entirely eustatic, between 2000 B.C. and 0 A.D. If our correlations are correct, Vero and Melbourne Man, as well as the Preceramic and Orange cultures, should have been in existence during the same period.

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*DIVISION OF MYCOLOGY**

MYCOLOGICAL PROBLEMS IN DETERIORATION OF MILITARY EQUIPMENT†

By WALTER N. EZEKIEL‡

Deterioration of military materiel during World War II resulted in recognition of a new border-line field of scientific knowledge concerned with such diverse matters as the etching of lenses of binoculars by fungus growth, the quick failure of signal equipments taken into the tropical jungles, the moisture and fungus damage that caused inaccurate aiming and erratic operation or failure of electrical power drives of anti-aircraft guns in the Philippines and aboard ship, as well as the more familiar rotting of cloth used in tents and for camouflage.

Because of the dramatic damage in the tropical areas, the term "tropical deterioration" was sometimes used to designate the trouble, and the accompanying term, "tropicalization," for preventive treatments applied in attempts to prevent the deterioration.[1] Information gathered since the war, to the effect that moisture and microorganisms, particularly fungi, can and do cause just as serious damage (if not quite so rapidly) in areas far from the equator, has made it desirable to drop the names mentioned in favor of terms such as "moisture and fungus proofing" and "deterioration prevention." It is important that people responsible for design and maintenance of some industrial and consumer goods, as well as of military items, should not believe that protection of this kind is necessary only for equipment destined for a tropical environment.[2]

Moisture and Fungus Damage to Materiel. The story of damage to materiel has been gradually becoming known since World War I, when trouble was noticed on optical instruments. Vision through binoculars and telescopes was obscured by lush fungus growth, films of moisture, and eventual etching of lenses. Again, in World War II, the Australians, for example, reported that many optical instruments operated only four to eight weeks in the New Guinea campaign before they became infested by fungi and that binoculars shipped from Australia were often infested before they could be issued from the depots.[3] The situation was better with our larger optical systems on the ships. Moisture-proof construction of naval optical units had been developed to meet this hazard, and there was relatively little deterioration of these reasonably well-sealed optical systems.[4]

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* The Section of Oceanography and Meteorology held a meeting April 25, 1950, on "Objective Methods of Weather Forecasting." This was a forum discussion and no abstract has been received.

† This paper, illustrated with lantern slides, was presented at the meeting of the Division, April 28, 1950.

‡ Bureau of Ordnance, Navy Department, Washington 25, D.C.

The fact that textile products and lumber are subject to decay was also known before the war, and a background of some practical answers in this field was fortunately available from prior agricultural and industrial research. Further research under the impetus of the war improved treatments to protect tentage, tarpaulins, sandbags, and camouflage materials from rapid decay. Complicated combined treatments were developed and embodied in specifications, so that canvas, for example, might simultaneously be made resistant to fungi, to fire, and to water, while being dyed to a desired color.[3]

More unexpected during World War II was the damage in connection with the increasing use of electrical and electronic communication, detecting, computing, directing, and other mechanisms used ashore and afloat. The Signal Corps and the Marines found that signal equipment taken into the jungles might go out of action within a few hours. Mold growth enveloped wires, and soft plastic parts took up moisture so that they warped out of shape and became conductors instead of insulators.

Emergency protective treatment was introduced in both maintenance and production as early as 1942.[1, 5] This "tropicalization" treatment consisted of coating all possible surfaces with a moisture-resistant lacquer or varnish which contained a fungistatic agent. In conjunction with other improvements, this superficial coating is still employed by most of the services.

Larger electrical units were also damaged. The Army Ordnance Department discovered that electrical portions of the power drive of the 90-mm. anti-aircraft guns in the Philippines were so damaged by fungi and moisture that operation became erratic and ineffective. Checking newer production still in this country, the same units were sometimes found to be profusely overgrown by fungi and with electrical values below those considered minimum for field operation.

Damage on shipboard electrical equipment was studied in 1945 in a survey of vessels at bases along the West Coast, from small LST's to battleships. Fungus growth was found in some of the electrical equipment on every vessel and in equipment going through the shops of every base. It was common in transfer switches, in gun directors, in the gun power drives, and particularly in the battle-telephone systems.[4] The obvious fungus growth and corrosion was in many cases connected directly with a story of operational difficulty. Damage had been found earlier[6] in the equipment of vessels from the North Atlantic run, as well as in those from the South Pacific. Some equipment became moldy while in shipment to naval bases in this country, so that overhauling was required before it could be installed on the vessels. An Air Force survey came back with a similar story[7] of considerable deterioration of equipment on planes and in storage at bases around the world.

In the years since the war, information has continued to accumulate on damage occurring in this country as well as elsewhere. Electrical units stored in Washington and Philadelphia have been seen with spectacular fungus growth. Equipment awaiting use at naval bases, or in transit, shows deterioration of the same nature as that seen on units from the South Pacific. Improvement to prevent such attack is therefore considered necessary as a regular part of production of military materiel.

Exploration of deterioration problems and some basic and more applied research on means of preventing the damage have been carried out by relatively small groups working primarily in military laboratories or in university and industrial laboratories under research contracts with military agencies. In the present paper, the writer does not intend to go into the detailed results of these researches (except as they may be pertinent to some particular discussion) but will present some generalizations, which are possible now as a result of the accumulated information, with regard to analyses of problems of deterioration and to action that may be attempted. These generalizations include the various effects of fungi and moisture on materials and equipment, the different approaches that may be followed in attempting to prevent damage, and the kind and reliability of procedures for research and later for routine specification tests that may be involved in determining whether a particular material or treatment attains the selected goal.

A Classification of Effects of Fungus Growth. Deterioration of materials and equipments can be classified as resulting from several different kinds of effects. Failure to visualize all of these possibilities may cause only partial analysis of a problem and lead to mistaken ideas as to the relative importance of protection of equipment from the combined action of fungi and moisture.

(1) *Visible decomposition and obvious failure, of a particular isolated material or of that portion of a larger unit upon which the fungus growth is seen.* This would appear to be the simplest type of microbiologic damage. It would cover such familiar examples as the decay of fabrics used for sandbags, under conditions that induced visible fungus growth and obvious shredding away of the fabric, or the visible decay and failure of lumber. The fungus growth itself need not necessarily be readily visible as the cause of such an obvious failure to fit this classification.

(2) *No visible decomposition but a demonstrable effect on one or more important properties of the affected material or part.* This classification would cover a wide range of effects on military material. Fungi may produce an obvious or less obtrusive growth without causing an obvious disintegration of the substratum but with effects, such as changes in strength, color, flexibility, or electrical insulating values, that make the apparently unimpaired substratum of significantly lessened usefulness.

Some examples of this kind of effect have already been mentioned. With optical instruments, growth over lenses obscured vision and thereby impaired operation of the instruments long before chemical action by the fungi had caused etching of the lenses. Another example is the moldy shower curtain, which may be discarded because of permanent discoloration some time before it need be discarded because of weakness. Similarly, projection screens are damaged by discoloration from fungus growth rather than by disintegration.[3]

Extensive studies by Luce and Mathes[8] have indicated that electrical values of hook-up wire, particularly surface insulation resistance, may deteriorate sharply before there is visible mold growth. This effect is considered to be due to subvisual growth, with the interlacing germ-tubes providing electrical leakage paths over the surface of the wire. Work with coated fabrics (such as those used in raincoats) has brought out the conclusion that fungus growth on and in such materials is damaging, not from direct loss of tensile strength, but as a result of loss of flexibility of the fabric, which causes it to crack more readily.[9]

(3) *Damage to portions of an equipment other than that upon which the growth occurs.* This is not necessarily accompanied by either kind of effect noted upon the particular part that supports the fungus growth. The part supporting growth may show neither obvious disintegration nor reduction

in its own required performance characteristics, yet the effect on other parts may be sufficient to prevent the unit as a whole from operating.

In the case of electrical and electronic equipment, damage of this kind is common. Such nutrient materials as braid-covered hook-up wire and relatively soft fabric- or wood-flour-filled plastics may support profuse fungus growth. It is well known that fungus growth is accompanied by excretion from the mycelium of various salts, of enzymes and other organic materials, including organic acids, and later of alkaline staling products. This complex of products of fungus metabolism has a profound effect on metal surfaces nearby, enormously accelerating corrosion. Furthermore, fungus spores produced on portions of units such as those mentioned may shed profusely over other portions and cause unexpected damage there.

This general damage to equipment is frequently seen on synchro motors and generators and on larger motors. Braid-covered wires leading into these units serve as a path for fungus growth. There is usually little material within the motor on which molds can grow, but the growth around the wire serves as a center around which rotor and stator surfaces become corroded.

An instrument observed in Washington showed profuse mold growth enveloping plastic and cotton-braid covered wire near the top. Showers of spores drifting down from these portions had settled over the lower portions of the instrument. Continued germination of these spores had resulted in numerous specks of corrosion over the aluminum surfaces, and had also produced a continuous weft of growth over the ceramic and bare chromel wire in heater elements near the base of the instrument.

More extreme general effects, as compared to the extent of the moldy portion of the unit, were seen by the writer in the case of a new fire-control unit returned to the manufacturer for rebuilding. This unit showed a relatively small amount of fungus growth on small lengths of hook-up wire and on plastic. But the mycelium extended to nearby bronze spring contacts, which had become severely corroded, as had also gears and other mechanical parts directly below the moldy portions, so that the unit was disabled. Exactly similar gears and other metal parts, at a distance from the moldy parts but within the same enclosure, were scarcely discolored. Thus, the severe corrosion which disabled this unit before it could be installed on the ship was due specifically to the effect of the fungus and not just to moisture.

Goals in Work on Fungus Resistance. There are several approaches toward increased resistance of materials and equipment to fungi and moisture. One obvious approach is to keep material dry all the time, thereby also preventing biological action. This is a large subject in itself, going into engineering design, hermetic sealing, desiccant units, ventilation of equipment, care in packaging, special maintenance, and the like. This engineering approach is not of primary interest here and need not be considered further, except to remark that it is generally not a substitute for inherent improvement of the parts of equipment, as will be discussed later. Even perfect gasketing and pressure-proof enclosure of instruments does not guard against breaking of the seal in military action (if not earlier, by vibration, accident, or carelessness) and rapid deterioration if the instrument has been built of parts that cannot withstand exposure to some moisture.

With increasing recognition of the need for fungus and moisture resistance on the part of materials used in equipment, there has been some confusion as to just what is wanted. In one specification, the requirement is that no "deleterious" fungi shall grow on the material covered, with no inkling as to what is considered deleterious, how it is to be recognized, or how a material can be tested for acceptability.

For use in research, and particularly as a goal for later specification test methods, several interservice committees have officially adopted a summary of views recognizing the need for clear designation of specific objects in work on fungus resistance. These statements have pointed to the following separate goals that may be pursued just within the scope of electrical equipment and that need to be covered by appropriate test procedures:

(1) Inherent nutrient inertness (which the writer has recently designated as "funginertness") of a material or component.[2, 10] This characteristic refers to the relatively permanent property of not supporting fungus growth because the material, part, or component in question does not furnish the necessary nutrients, as distinct from resistance due to the presence of fungistatic or fungicidal agencies.

(2) Fungistatic (and/or fungicidal) effectiveness within materials or components.

(3) Fungistatic effectiveness of coating materials (varnishes or lacquers) to be applied over equipment and requiring test prior to such applications.

(4) Fungus and moisture resistance of a completed equipment or assembly. (This would be the final goal of the approaches above, but it would be determined by a different kind of test.) Tests of this kind cover general effects on equipments as a whole, which are not necessarily apparent from the work on the isolated materials or components.

This list could be extended by pointing to considerations of ability of a material or component to maintain a particular property, such as electrical insulation resistance,[8] in the presence of moisture and fungus growth.

The diversity of kinds of fungus resistance can be illustrated by the case of electrical insulated wire. Recently, use of wire with an extruded nylon jacket, instead of a braid, has been encouraged, partly on account of its fungus resistance.[11] The nylon jacket is funginert and also maintains good electrical properties in the presence of moisture. Still older specifications called for fungistatic coating of braid-covered wire, with appropriate tests to prove presence of the fungistatic chemical.[12] Recognition of the newer goal led to adoption of an appropriate test.

Test Procedures and Specifications. This discussion has gone into the recognition of the kind of fungus resistance desired and the selection of research procedures, specification requirements, and tests that will approach the desired goal. It is particularly unfortunate when the body of a specification announces that a material is desired that does not support growth (*i.e.*, one that is funginert), while the test method given requires presence of an active fungistat.[13] A first consideration with test procedures, then, is clear decision as to what is desired and how it can be recognized. Examples of fungus resistance tests aimed at particular goals are available in some specifications.[11]

Another consideration particularly applicable to the official specifications or contracts by which particular products are purchased or manufactured is the practical recognition of what can be obtained. For procurement at a particular time, there is little point in a specification requirement that is a research goal and for which the answer is not known. From the viewpoint of both research and procurement, it appears desirable to separate clearly these two matters.

The reliability of test procedures in these relatively new areas of mycological work is another matter in which attention is necessary. Consistency of results by a particular laboratory procedure is no insurance that the true answer is being obtained. For example, in considering fungistatic agents for coating materials to be used on electrical equipments, it has been discovered that ratings by a rather well-standardized laboratory procedure do not agree with field exposure tests.[14] Of three fungistatic agents often used, pentachlorophenol was effective by laboratory qualification but in field exposure of treated material proved to be fungistatic for only a brief period. Phenyl mercuric salicylate was outstanding under laboratory conditions, with thermostability suggesting a long life, yet under field conditions it was not effective for as long a period as salicylanilide, which had barely qualified in the laboratory. These results point to a need for consideration and selection of these particular materials by comprehensive study beyond that possible in the prior routine specification test.

Importance of Practical Application. Despite the need for further information in many directions, which is obvious from the general matters discussed, it is desirable to apply immediately the concepts of resistance to moisture and fungus damage. Particularly for military equipment, the practical benefit to be obtained by selection of the better of materials available is so great that this should be done without delay. In many cases, it may be possible to obtain considerable improvement at little if any additional cost simply by excluding some of the poorer materials that are still being used.

Summary. (1) Fungi and moisture have caused extensive damage to military materials and equipment. Deterioration of this kind was common during World War II in tropical areas and also on similar units in this country and in vessels from the North Atlantic run.

(2) Fungus damage may consist of visible decomposition and obvious failure of particular materials or parts; degradation in some important characteristic without visible decomposition of the affected material or part; and damage to portions of an equipment beyond that upon which the growth occurs.

(3) The necessity is pointed out for clear recognition of general goals, such as funginertness as compared to fungistatic or fungicidal properties of a material. Research and specification test procedures should be appropriate both to the general approach and to the particular material.

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SECTION OF BIOLOGY[*]

SEMINAL VESICLE RESPONSE AS AN ASSESSMENT OF ANDROGENIC ACTIVITY[†]

By J. H. LEATHEM[‡]

The response of the rat seminal vesicle as a possible assay end point for androgenicity received considerable impetus from the reports of Hays and Mathieson[1] and Wills, Rampton, and Pugsley.[2] A maximal seminal vesicle weight increase was obtained 72 hours after an injection of 0.25 mg. of testosterone propionate in castrated rats. A seminal vesicle weight increase of the same degree can be induced in 22-day-old normal rats. The response is not influenced by the volume of the vehicle but is altered by the route of administration.[3]

Testosterone propionate will quickly increase the oxygen consumption, fructose content, water, and weight of the seminal vesicle of castrated rats.[4] In normal rats, we have found that 0.25 mg. of testosterone propionate increased seminal vesicle weight 214 per cent in 72 hours and increased water content from 78.0 to 82.0 per cent. This steroid will restore the alkaline phosphatase enzyme concentration to normal levels from the subnormal levels of the castrate. Both chemical and histochemical evidence support the androgen-enzyme relationship.[5, 6] In general, alkaline phosphatase enzyme in the rat seminal vesicle is concentrated in the subepithelial stroma, with little[7] or none[8] in the epithelium. After 3 hours' incubation in glycerophosphate, the normal 22-day-old rat gland exhibited some slight nuclear reaction in the epithelium, but the major concentration was seen in the subepithelial stroma. No change in enzyme distribution was observed 72 hours after injection of 0.25 mg. of testosterone propionate.

A definite weight response can be induced with 50 micrograms of the androgen in castrated rats.[*] We have studied three groups of 22-day-old normal rats after dosages of 25 and 50 micrograms given as a single subcutaneous injection. After 72 hours, seminal vesicle weight was 35, 54, and 87 per cent above control weight with 25 micrograms and 57, 105, and 110 per cent above control weight with 50 micrograms.

Since a 200 per cent seminal vesicle weight increase can be induced 72 hours after 0.25 mg. of testosterone propionate administration, it was of interest to test other androgens in the 22-day-old rat. Single subcutaneous injections of 0.25 mg. of testosterone, androsterone, dehydroisoandrosterone, epitestosterone, vinyl testosterone, androstanediol 3, 17 diacetate failed to induce a response, but testosterone hexahydrobenzoate did increase seminal vesicle weight 100 per cent. Increased dosages of varied androgens were studied to determine their relative androgenic action on the rat seminal vesicle, and these data are presented in TABLE 1.[†]

These data are of interest initially in comparison of testosterone, androsterone, and dehydroisoandrosterone. Parkes and Emmens[9] compiled a table of androgenic activity as gained from capon comb response and found that 15 micrograms of testosterone induced the same response as 100 micrograms of androsterone, whereas 200 to 300 micrograms of dehydroisoandrosterone was needed to duplicate the action. When these same androgens were studied in the rat, the seminal vesicle stimulating activity appeared to be virtually the same (TABLE 1).

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[*] The Section of Geology and Mineralogy held a meeting on May 1, 1950, at which Matt Walton read a paper entitled "The Blashke Island Ultrabasic Complex, Southeastern Alaska." No abstract of this paper has been received.

[†] On May 8, 1950, the Section of Biology held a symposium, presided over by W. W. Scott of the Johns Hopkins University School of Medicine, on "Recent Studies on the Mammalian Male Sexual Accessory Glands." This paper, illustrated with lantern slides, was the first of four presented at the symposium.

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[*] This investigation was supported in part by a grant from E. R. Squibb & Sons to the Protein Metabolism Fund of the Bureau of Biological Research, Rutgers University.

[†] The androgens were generously supplied by Ciba Pharmaceutical Products, Summit, New Jersey.

Kochakian[10] found that the percentage increase in combined seminal vesicle and prostate weight was the same 30 days after pellet implantation of androsterone and dehydroisoandrosterone but that testosterone was much more active.

With the exception of testosterone, more general agreement was observed in comparing capon comb and seminal vesicle weight response. In the capon, the following androgens are listed in descending order of their activity: testosterone, methyl-dihydrotestosterone, dihydrotestosterone, methyl testosterone, androsterone, dehydroisoandrosterone and cis-testosterone.[9] In the rat, methyl-testosterone may be slightly more active than 17 methyl-dihydrotestosterone and both were more active than dihydrotestosterone. Vinyl testosterone was more active than testosterone which, at best, was little more active than androsterone and dehydroisoandrosterone. Epitestosterone has little activity in the capon, and no response was obtained with 3.5 mg. in rats. However, esterification clearly increased the activity of this steroid.

TABLE 1

SEMINAL VESICLE WEIGHT RESPONSE OF 22-DAY-OLD RATS 72 HOURS AFTER A SINGLE SUBCUTANEOUS STEROID INJECTION

Steroid	*Maximum dose injn mg*	*Sem ves increase %*
Testosterone prop.	0.25	200+
Testosterone hexahydro-benzoate	0 25	100
Methyl testosterone	0 75	63
17 Methyl dihydrotestosterone	1 0	43
	2 0	133
	3 5	215
Dihydrotestosterone	1 0	48
	3 5	93
	5 0	192
Vinyl testosterone	2 5	60
	5 0	100
Testosterone	3.5	70
Androsterone	3 5	60
Dehydroisoandrosterone	3 5	60
Epitestosterone	3 5	0
Epitestosterone prop.	2.5	27
	3 5	93
2 Bromo-testosterone hexahydrobenzoate	2.5	0

Summary. An increase in seminal vesicle weight of the immature rat is readily obtained 72 hours after a single subcutaneous injection of testosterone propionate. Water content of the glands is increased, but histochemical alkaline phosphatase concentration is not changed. Assessment of other androgens permits some estimation of comparative androgen activities and indicates that differences exist between capon comb and seminal vesicle responses.

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THE RESPONSE OF THE MALE MOUSE SEX ACCESSORIES TO TESTOSTERONE DURING INANITION[*,†]

By E. D. GOLDSMITH[‡] AND R. F. NIGRELLI[§]

A number of investigators have demonstrated that optimum growth in several groups of animals requires adequate supplies of pteroylglutamic acid (PGA). Folic acid has been shown to be essential for metamorphosis[1] and growth[2] of several species of insects. Goldsmith and Harnly[3] have reported that both a crude antagonist (X methyl PGA, Lederle) and an antagonist of known chemical structure, 4-amino PGA (Aminopterin, Lederle), prevented the development of the larvae of the fruit fly, *Drosophila melanogaster*, into adult flies. The inhibitory effects have been overcome with large amounts of PGA and much smaller quantities of desoxyribonucleic acid.[4] Chicks and monkeys maintained on a diet deficient in PGA showed a decreased growth response of the female genital tract to estrogens.[5] A PGA deficiency produced by the administration of PGA analogues has resulted in a reduced response to female sex hormone of the oviducts of the rat,[6] chick,[6, 7] and frog.[8, 9]

Since PGA is apparently required by diverse tissues for their optimum growth, it was intriguing to explore the growth reactions of the sexual accessory glands of testosterone-treated castrate male mice maintained on a ration containing a PGA antagonist. Extensive experiments revealed that the seminal vesicle-coagulating gland complex of the castrate mice fed a Purina chow diet containing either X methyl PGA (Lederle)[10] or 4-amino-N^{10}-methyl PGA (Amethopterin, Lederle)[11] did not grow as well as did those in the mice on the control stock diet. Pretreatment with and administration of large quantities of PGA concurrently with the Amethopterin negated the action of the antagonist.

Although the evidence in favor of the view that folic acid was required for the satisfactory growth response of the male accessories to the male sex hormone appeared ample, the considerable weight loss of the antagonist-treated mice made it necessary to ascertain whether the observed reduced growth response of the accessories of this group to testosterone might be due not to a lack of folic acid *per se* but rather to the concomitant inanition. It is true that it had been reported previously that inanition in the rat[12, 13] did not alter the potentiality of the sex accessories to respond to either gonadotrophin or testosterone. Additional evidence supporting this finding is to be found in the work of Mason and Wolfe,[14] and Werner.[15] Studies on the chick,[16] guinea pig,[17] and man[18] have also yielded similar results. Nevertheless, as a further check, it was considered desirable to investigate the reaction of the accessory sexual glands of the castrate mouse to testosterone during inanition.

Twenty-eight to thirty-three day-old CFW mice were castrated and were placed on a Purina chow ration for 20 days. The animals were then divided into 3 groups: (1) and (3) were fed *ad libitum* (approximately 4 gm. per day); (2) was restricted to 1 gm. per day. A fourth group was composed of normal non-castrates of the same age.

In TABLE 1, the results of a typical experiment are tabulated. After 7 days, the mice in group 2 had lost about ¼ of their total body weight. At this time, daily parenteral administration of 1 mg. of testosterone propionate[*] in 0.04 cc. of sesame oil or an equivalent quantity of sesame alone was initiated. All animals

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[*] On May 8, 1950, the Section of Biology held a symposium, presided over by W. W. Scott of the Johns Hopkins University School of Medicine, on "Recent Studies on the Mammalian Male Sexual Accessory Glands." This paper, illustrated with lantern slides, was the second of four presented at the symposium.

[†] This investigation was supported in part by a research grant from the National Cancer Institute of the National Institutes of Health, U S. Public Health Service.

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[*] The testosterone propionate (Perandren) was generously supplied by Dr. F. F. Yonkman and Dr. F. L. Mohr of Ciba Pharmaceutical Products.

TABLE 1

UNDERFEEDING AND THE RESPONSE OF SEXUAL ACCESSORIES OF CASTRATE MALE MICE TO TESTOSTERONE

*Group**	*No. survivors per group*	*Procedure*	*Orig. body wt. (gm.)*	*Final body wt. (gm.)*	*Accessory wt. (mg.)*	*Accessory wt. (mg.) / 100 gms. body wt.*
1	14	Fed *ad lib.* 11 days. 1 mg. testosterone injected daily last 5 days	18.9	23.2	55.3	237.7 ±42.2†
2	30	Fed ¼ *ad lib.* 11 days. 1 mg. testosterone injected daily last 5 days.	21.4	13.9	14.1	102.2 ±18.7
3	8	Fed *ad lib.* 11 days. Sesame oil injected daily last 5 days.	18.0	23.5	6.4	29.6 ± 3.0
4	5	Fed *ad lib.* No treatment	21.7	24.1	205.3	865.5 ±73.1

* Groups 1, 2, and 3 composed of animals castrated at 28–33 days of age. Experimental treatment instituted 20 days after castration. Group 4 mice were normal non-castrates.

† Average deviation.

were sacrificed 24 hours after the fifth injection.

Seminal vesicles, coagulating glands, and prostates were removed, weighed as a unit on the torsion balance, and prepared for histological examination. When the experiment was terminated, 50 per cent of the mice in group 2 had died and the survivors had lost about ⅓ of their initial body weight. Both the fatalities and weight loss were comparable to those observed in animals treated with Amethopterin during a similar period.

Examination of the table reveals that both on an absolute and on a 100 gm. per body weight basis the accessory genitalia of the mice on the restricted diet were significantly lighter than those fed *ad libitum.* Histological examination demonstrated that the seminal vesicles were juvenile in appearance and contained no secretion. The glandular epithelial cells were lowest in the inanition group. The periurethral glands, which serve as an excellent indicator of testosterone activity, were absent in these animals.

It would thus appear that the response of the accessory sex organs in the castrate mouse to a sufficient supply of male sex hormone is governed by an adequate food intake. It is hoped that completion of experimental work, now in progress, utilizing purified diets deficient in folic acid or pyridoxine, and the treatment of starved animals with the several factors of the B complex, alone and in combination, may further clarify the situation.

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THE INFLUENCE OF CERTAIN STEROIDS ON THE BEHAVIOR OF HUMAN PROSTATIC CANCER*

By J. B. TRUNNELL AND B. J. DUFFY, JR.†

Ten years have passed since Huggins[1] and Herbst[2] first demonstrated the spectacular improvement which follows castration or estrogen therapy in patients with cancer of the prostate gland. The decade, however, has been one of sharp contrast between bright hope and keen disappointment. In a recent review, "Chemotherapy of Malignant Disease," Gellhorn and Jones[3] have said, "to date androgen control therapy of prostate gland cancer has provided the single, most outstanding, practical contribution to the chemotherapy of malignancy." A consideration of the five-year survival data in a large series,[4] however, calls forth real doubt that these measures significantly prolong life, a fact which only heightens the need for investigation in this fallow field.

Failure to understand the *modus operandi* of control in these cases hinders one in comprehending the now crucial problem of relapse. Several hypotheses have nevertheless been advanced to explain in general terms these closely integrated phenomena. The most popular explanation is that advanced by Huggins.[5] This investigator postulated that the removal of androgen producing tissue or the peripheral neutralization of androgens deprives prostatic tumor cells of a substance essential to their prosperity. He explained relapse in terms of either (1) increased production of adrenal androgen, or (2) the development of "androgen independence." A few patients in relapse have been subjected to removal or irradiation of the hypophysis or of the adrenal glands. The present impracticability of such surgical procedures and the inconsistency of the radiation results led us to search for more physiologic means of inhibiting androgen production.

Albright[6] has proposed that androgen production by both the adrenals and the testes is largely controlled by the luteinizing or interstitial cell-stimulating hormone (LH or ICSH). It has long been believed that the output of a pituitary trophic hormone is inhibited by the hormone of the gland which it stimulates. In accordance with this principle, androgens and progesterone are both thought to depress the output of LH in normal endocrine physiology. Exogenous androgen or progesterone might therefore be expected to reduce the availability of LH and, secondarily, to reduce the elaboration of endogenous androgens.

In the interest of keeping the total (exogenous and endogenous) androgen at a minimum, progesterone, though slightly androgenic, was selected for initial clinical trial.* This compound was administered in doses of 25–300 mg. daily, either as an aqueous suspension or dissolved in oil. Fifteen patients have been so treated. Eight of these were previously untreated, 7 responding well. The single treatment failure, subsequently, also was not benefited by castration. Seven other patients were in spontaneous relapse following remission induced by castration or by estrogen therapy. Six of these also responded favorably to progesterone administration. One or more patients showed disappearance of pain, return of appetite, weight gain, correction of anemia, relief of urinary obstructive symptoms, return to gainful employment, and lowering of the serum acid phosphatase levels, accompanied by an alkaline phosphatase flare.† Whether the reasoning which led to its trial was correct, these experiments demonstrated progesterone to be an effective therapeutic agent in the treatment of prostatic cancer.

A troublesome pharmacologic problem

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* On May 8, 1950, the Section of Biology held a symposium, presided over by W. W. Scott of the Johns Hopkins University School of Medicine, on "Recent Studies on the Mammalian Male Sexual Accessory Glands." This paper, illustrated with lantern slides, was the third of four presented at the symposium. A fourth paper, entitled "Further Hormonal Relationships in Prostatic Growth," was presented by Doctor Scott. No abstract of this paper has been received.

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* Supplied through the courtesy of Dr. E. C. Reifenstein, Jr., of Ayerst, McKenna, and Harrison, Ltd., and Dr. Edward Henderson of the Schering Corporation.

† During initial stages of favorable response, patients with bone metastases often show a rise in alkaline phosphatase before the level of this enzyme begins to fall. This is thought to be a function of bone regrowth in perimetastatic areas.

arose, however, in that the majority of patients developed painful induration at the site of intramuscular injection. In an attempt to resolve this difficulty, other routes of administration were employed. Taken orally, progesterone is relatively inactive. In the female, however, pregneninolone (anhydro-hydroxy-progesterone or ethinyltestosterone) by mouth is an effective progestational agent. Under the latter circumstances, it is felt to possess only about one-tenth the activity of parenterally administered progesterone. It was in the use of pregneninolone that we first observed evidence of pluripotential response. Several patients improved dramatically on oral pregneninolone therapy (25-100 mg. per day), whereas other patients receiving similar doses grew rapidly worse, with rises in the serum acid and alkaline phosphatases accompanying clinical deterioration. There seemed little doubt that pregneninolone had a variable effect on prostatic cancer.

A similar dual-effect potentiality was then observed in a patient receiving progesterone parenterally. He had been controlled with 200 mg. per day (intramuscularly in oil), but, when a total of 1500 mg. progesterone pellets was implanted subcutaneously, he became rapidly worse. Pain returned almost at once, and the phosphatase values rose to very high levels. On a dosage-response basis, pellets, because of constant, slow absorption, are much more effective than single daily injections. It appeared, therefore, that in this patient, whose cancer had been suppressed at one dosage level of progesterone, there was unquestionable stimulation at a higher level.

Having observed this behavior in two compounds, we began to wonder if the same tendency might be observable in the use of estrogen or of testosterone. The latter substance has often been said to stimulate the growth of prostatic cancer to a marked degree. As mentioned, however, testosterone should theoretically be capable of decreasing endogenous androgen production, and, if this result could be achieved with small quantities, the total (exogenous and endogenous) androgen level might be lowered sufficiently to inhibit tumor growth.

We were rather surprised to find that only 6 out of 21 patients could be made to exhibit clear-cut evidence of tumor stimulation when given testosterone. In fact, one patient received 100 mg. testosterone propionate daily for one month without stimulating effect. Indeed, Brendler[7] has reported testosterone therapy of 3 patients with prostatic cancer with clinical improvement. Some of our patients, as well, have claimed to feel better following injection of small doses (10–15 mg.) of testosterone, but we have seen no corresponding alteration in the serum acid phosphatase level. The latter failure may be attributed to the fact that patients so studied in our clinic were seen at weekly intervals. Hence, it is possible that transient lowering of phosphatase values occurred but were not measured. It may be mentioned in passing that those patients who did exhibit sensitivity to testosterone (were made worse) showed an improved response to estrogens following such stimulation. Taken as a whole, these observations on the influence of testosterone do not convincingly demonstrate the divergent capabilities (stimulation and control) clearly shown following treatment with either progesterone or pregneninolone. They suggest, nevertheless, that under appropriate circumstances such demonstration may be forthcoming.

It has been even more difficult to obtain evidence that, at high dosage levels, estrogenic substances might also stimulate prostatic cancer activity. In fact, at least one group of investigators[8] has failed to observe stimulation with doses of 500 mg. of stilbestrol per day over periods of several months. Scott,[9] on the other hand, has seen increased pain and elevation of acid phosphatase values in a patient given estrogens. These changes were reversed when estrogen administration was discontinued. We have observed similar changes in a patient who received 680 mg. estrone* intravenously together with 96 mg. ethinyl estradiol orally during an 8-day period. He was subsequently brought under control with progesterone.

Seyle[10] has called attention to a "periodic table of the steroids" in which, like the chemical elements, he has arranged certain steroid hormones according to their biological activity. Our experience with four steroid hormones (namely, estrogen, progesterone, pregneninolone, and testosterone) has led us to feel that, in the order named, these substances may be similarly arranged, and that further, probably as a function of dose size, prostatic cancer appears to be either controlled or stimulated by each compound. In other words, following administration of the compounds named above, eight responses are predictable. Actually, we have observed six of these responses and even have some inconclusive, but suggestive, data concerning the remaining two (stimulation with large doses of estrogens and inhibition with small doses of testosterone).

The foregoing concept, which is considerably less rigid than the older "androgen neutralizes estrogen" idea, serves as a starting point for possible explanation of some heretofore perplexing aspects of therapy of this disease. In order not to be prematurely lured into serious consideration of these explanations, however, we must remain cognizant that an imposing number of other variables exists. In mentioning only the more obvious of these, we may list competence of hepatic functions; duration and extent of the cancer; presence or absence of the testes; and, perhaps most important of all, the availability of certain essential nutritional factors. In the latter connection, we have been intrigued by reports of Hertz[11] and others, who have shown that folic (pteroylglutamic) acid deficiency will not permit estrogen to exert its usual growth stimulating influence on the chick oviduct. These workers have shown that the estrogen effect may be prevented equally well by a folic acid-deficient diet or by a folic acid antagonist. In either instance, the administration of folic acid restores incompletely the ability of the oviduct tissue to respond to estrogen. We have twice observed in one patient that the folic acid antagonist, a-methopterin (4-amino-n-methylfolic acid) apparently deprived estrogen of its ability to control prostatic cancer. This man was fairly well controlled with estrogen but had slight elevation of acid phosphatase. After 14 days of a-methopterin (5 mg. per day orally), he exhibited a sharp increase both in pain and in phosphatase values. These subsided after stopping administration of the antagonist and were subsequently reproduced during a second course of its administration.

The obvious implication of these results is that folic acid may be required for optimum therapeutic results with estrogen. We have, however, been unable in two instances of spontaneous relapse on estrogen to restore a state of control by administration of even large doses (250 mg. per day) of folic acid. These patients claimed to feel better, but their claims were not supported by decreases in acid phosphatase levels. Hertz,[11] on the other hand, was also unable to obtain complete restoration of activity (oviduct response) by the addition of folic acid to a "purified" diet. He postulated that one or more other substances might have to be supplied in addition. His observations suggest that pantothenic acid, riboflavin, and pyridoxin are *not* specifically required for estrogen action, but that liver contains agents capable of restoring full estrogen reactivity of the oviduct. We are currently investigating the influence of liver extracts on the quality and duration of estrogen induced remissions.

In conclusion, it must be confessed that whatever light our observations have shed upon this fascinating problem has served only to illuminate its enormous complexity.

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* Premarin solution supplied by Dr. E. C. Reifenstein, Jr., of Ayerst, McKenna, and Harrison, Ltd.

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SECTION OF PSYCHOLOGY

PSYCHOLOGY IN MODERN MANAGEMENT[*]

By MARION A. BILLS[†]

What are the functions of an industrial psychologist? As I see them, they are (1) to use the special knowledge which our training as psychologists has given us to advance better management; (2) to increase that store of knowledge; and (3) to make available to qualified users the results of our increase in knowledge.

In what fields of management should the psychologist work? The study of psychology is the study of humans. Therefore, any phase of management which involves people is a legitimate field in which we as psychologists may function: employee selection, job evaluation, merit rating, time and motion study, incentives, machine design, training, promotion, counseling, job satisfaction, wages, vision, recreational activities, communications, costs, *etc.* Every one can add from his own experience other items to this list. This wide range of possible activities has been both a handicap and an advantage. The physician, the engineer, or the lawyer have their responsibilities better defined. Specialists in their field are recognized. Twenty-five years ago, the industrial psychologist had to function both as a general practitioner and a specialist, for we had no specialist to whom to turn. Today, this is less true. The industrial psychologist today functions in the capacity of a general practitioner, and, as it is for any practitioner, it is our duty to diagnose the case, conscientiously work out the treatment (using specialists, when necessary), and sell that treatment to management.

Some examples from my own experience may illustrate how it seems to me a general practitioner should work. First, a problem on which we have been working for many years, and intensively now for several months: the better selection of life insurance agents. In meeting this problem, our first question was: have there been any methods already worked out which have stood the test of time, or must we produce something new? As a whole, psychologists have been less inclined than the medical or engineering profession to use the information that we have and more inclined to want to try something different. Perhaps this is the natural fault of a young profession. Until we learn to draw to the fullest extent on our backlog of knowledge, however, we will not become a mature profession.

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[*] This paper was presented at the meeting of the Section, May 15, 1950.

[†] Aetna Life Insurance Company, Hartford, Connecticut.

In the selection of life insurance agents, the literature showed that two methods of selection had worked with reasonable success, Strong's Interest Test, scored for life insurance, and a weighted application blank. The next question was: will these methods predict success in our particular firm? We decided two years ago that we would make an intensive study of Strong's Interest Test. The Life Insurance Agency Management Association was making a study of the weighted application blank, and we knew that we could trust their results. In 1948, we gave the Strong's Interest Test to all full-time agents hired. By the end of 1949, the "A" men had an average production of 3.5 times the low-scoring group, and 53 per cent were still employed. The "B+" men had an average production of 2.1 times the low-scoring group, and 43 per cent were still employed. Of the low scoring group, only 21 per cent were still employed.

This data convinced us that the employment of "A" men was definitely better management, but, in the life insurance business, one must sell not only an Agency Department, but agencies and managers, spread all over the United States and Canada. All of this was interesting data to them, but it was simply data. How should we say it to bring it home to them? Here is the way we stated it. If an agency wanted from its new men a million-dollar production by the end of the following year, they would need to hire, in a given year, on the average of 4.8 persons scoring "A," 7.8 persons scoring "B+," or 16.7 persons scoring lower. This is just a re-arrangement of the data, but it is something that can be remembered easily. The picture becomes vivid to the manager. He sees 16 men for whom he will have to provide office space (and rents are high), a desk, and chairs. He sees 16 men come and go in and out of his agency. He must educate them, train them, counsel them on how they are using the training, listen to their disappointments and troubles, give them pep talks, re-educate them, and, in some instances, give added financial assistance. He also sees too large a proportion of these men being unsuccessful and he realizes his responsibility to society in not encouraging men to go into the business whose chances of success are low. When he really thinks this out, and the figure of 16 men helps him to do it, he tends to vote for the 4.8 better men instead.

The Life Agency Management Association had, meanwhile, made a detailed study, in which we cooperated, of a weighted application blank. Adding this information to our study, we can say to the manager, "If you want a million-dollar production by the end of the following year, you will need to hire 4.3 men scoring high in both Strong's Interest Blank and the weighted application blank, but you will need to hire 22.9 men scoring low in both. That is, you will have to hire five times as many." We feel that this is as well as we can do at present with objective means; it is as well as most tests predict. Our next step is to turn to some of the more subjective selection approach.

First, the interview. In testing this, we have had experience. Each year, we hire a number of casualty special representatives who, after training in our home office schools, enter our agencies as liason between our commission agents and the agency. In 1946–47, these men were being hired by our managers in the field, and each man interviewed was rated by the interviewer on 10 qualities chosen as essential to the job. A year later, the managers rated the individuals on their success as special representatives. In 1948, a study of these two ratings showed no relationship. Of the 150 men studied, however, 41 had been interviewed by a home office representative who had wide experience in interviewing, and for these there was a definite positive relationship between his interview rating and the rating of the manager at the end of a year. Based in part on these results, we changed our procedure. In 1949, no special representatives were hired without an interview by an experienced home office representative. This was costly, of course, but in 1949 our experience was sufficiently better, so we are continuing the new procedure. It was feasible for the small number of special representatives which we hire. It is not feasible for the large number of life agents whom we contract each year. Therefore, it becomes a problem of training our 150 to 200 managers and supervisors in interviewing methods.

One probably thinks immediately of the patterned interview. At present, we use a patterned interview for the selection of our life agents. It has, I think, been useful for two reasons. First, it outlines in detail information that must be obtained. That information is valuable, and, in getting it, we slow down the interview so that more time is spent with each applicant. But, while the patterned interview tells the interviewer what information he should get, it does not train him in the methods of getting that information and in the subtle means of interpreting that information. The full technique can come only by experience, but experience itself does not make a good interviewer. Bad habits are so easy to acquire in interviewing that persons often become increasingly poorer interviewers with experience, especially when there is no check on interviewing results. We are struggling at present with the idea of typical transcribed interviews to which our supervisors may listen and give their criticisms and their reactions, which can then be directly checked with actual results.

A second source of subjective-selection information is recommendations. Although I have no conclusive results as yet, I will describe our present approach to this subject, since it illustrates another way in which the industrial psychologists may work in helping to organize material already in the minds of management (let me add here that many business executives are very good psychologists).

After a meeting at which the value of recommendations was discussed, one of the sales managers present said that he felt one could interpret a recommendation by the adjectives used, even though on the surface they all seemed good. This to him was simply a hunch, and, if it was to be useful for others, it had to be systematized.

We started the study by reading over several hundred recommendations. One of these recommendations still sticks in my mind. It was a neighbor trying very hard to recommend a young college man, but trying also to be honest. After several paragraphs of factual matters, he came up with, and you could almost hear a sigh of relief, a positive statement, "He is a good dresser." From the study of these recommendations, we drew off 20 words which were often used—all favorable. With the 20 favorable items listed, we then asked a group of our supervisors each to choose 10 people, 5 whom they considered good salesmen and 5 not so good, and check a blank for each of them. We headed our list "Please indicate, by underlining, the 5 adjectives which you feel describe this individual most accurately. Please underline 5 only."

In our sample run, we found a difference in the items checked for the successful agent and the unsuccessful one. Of course, certain of the qualities were neutral. For a year now, we have attached this form to our requests for recommendations and a year from now we hope to have definite information on it.

Not all of an industrial psychologist's problems are in the selection field, so I will choose as a second illustration a problem in a different field. As is true for all companies, not only do we want our company to be a good place to work, but we want our community to recognize us as such. What makes a job desirable? Most of the literature on the subject has been from manufacturing concerns. We felt that a clerical organization might give different answers. Also, we wanted the opinion of our own employees. We asked a fair sampling of them to fill out a blank, headed "What Makes One Job More Desirable Than Another." It listed fifteen items, taken mostly from other surveys. It was not an attitude survey. We did not ask the individual, for example, whether he considered that the policies of our company were good or bad, but only whether he considered them important. The importance of the items came in order: "a fair boss," "work which interests one," "opportunity for getting ahead," "good pay," and "security."

Since "good boss" got the first place, the questions arose, "what makes a good boss" and were our bosses filling the bill? For an answer to this, we turned to a specialist. For two summers, we had a person trained in interviewing come in and interview employees chosen at random. The topic of the interview was how they liked their job, but the boss came into practically every discussion, and we believe the opinions expressed were quite frank and uninhibited, since they were given to an outsider and their highly confidential nature was stressed. The third summer, our specialist interviewed our supervisors. The information of the first two summers was given to our supervisors in generalized terms, and, by talking it out, they were able to see its application in detail. We believe that our supervision has improved.

A few of the items that came out regarding what makes a good boss may be of interest. A good boss expects results, but he doesn't insist on being the only boss. He lets you boss your own job. He doesn't check progress too often. He isn't always peering over your shoulder. He gives praise in public when due; criticism in private. He takes your requests seriously, even though he has to turn them down. He sees that the work is explained to you, and he tells you, or at least lets you know, whether it is good or bad.

Perhaps the most pleasing result of this survey was the fact that a majority of our employees wanted to be the boss of their jobs. They wanted to make at least the minor decisions themselves. That is the American way. This desire to boss one's own job is one which should be kept alive in our world of industry, and anything which we as psychologists can do to further it is more than worth the effort. I want to digress a minute, but it is really simply to stress this point.

A little more than 25 years ago, the Ætna Life Affiliated Companies realized that they had grown too large for the miscellaneous salary structure, which was influenced largely by each supervisor's own opinion, and decided in fairness to all employees to work out a job evaluation. We used a classification system based largely on complexity of work which had been developed by a group at the Bureau of Personnel Research at Carnegie Institute of Technology. The choice of a system was less difficult then than it would be now. Point evaluation and factor weighing systems were yet to come into the picture. This classification method seemed to fit our needs and, with minor changes, is the one we use now for our entire home office staff. In general, it breaks down into three groups of jobs, with several steps in each group. First, there are the jobs where there are only a few rules and regulations to follow and where all of these rules and regulations are definite and specific. The second group is where there are a large number of rules and regulations to be followed in carrying on the work, but, again, these rules and regulations are definite and specific. The third group are the decision-making jobs. To those who think of a large office as made up of a number of standardized jobs where people add figures, file cards, or check reports, it may come as a surprise that 45 per cent of our jobs are decision-making ones. Let me give you a typical example of such a job. Its title is Life Underwriter. The man holding this job must make his decision, from the papers submitted by the agent to the home office, whether or not to accept the individual as a risk. That is, this man, on his own responsibility, can and does commit the company. For this group, we do not need to worry that they are experienced in making their own decisions.

Probably at least one-third of the persons holding a job where rules have to be followed are headed for decision-making jobs

They are learning to make decisions. This leaves us approximately 35 per cent where the supervisors must take special pains to see that the individual realizes what is true, that he is the boss of his own job. The ways of doing this are numerous. Let me quote one out of the many which we use: measured production with pay for meeting or exceeding the accepted minimum established. For example, the woman who has turned out 1500 lines of typing in a day, instead of the minimum of let us say 1000, has bossed her own job and has budgeted her own time; she is proud of it and she is paid for it. She is not, and realizes that she is not, simply a cog in the wheel.

I would like to give you an illustration in the field of clinical psychology. I am not a clinical psychologist and I make no pretense of being one. As the only psychologist in an organization which did not have a psychiatrist until recent years, however, mental and emotional maladjustments which present themselves in any firm gravitated to my sphere. The first problem was to convince management that mental disease should be viewed in the same way as a heart attack or tuberculosis. Not all cases recover, but many do. The second problem was to hunt for reliable outside sources and, through them, to train our supervisory group to recognize early stages of mental and emotional maladjustments, and to use to the full these outside resources for the help of individuals in need. Help can come from psychiatrists, clinical psychologists, ministers, priests, clinics, and such organizations as Alcoholics Anonymous. We used them all at times and found them most willing to cooperate. We still use them, though three years ago we met the problem head-on and added a psychiatrist to our medical staff.

Following the advice of the old colored preacher who, in describing his sermon said, "I tells them what I'm going to tell them, then I tells them, and then I tells them what I told them," I want to review the points which, directly or indirectly, I have tried to make. First, as industrial psychologists, it is our duty to study and use all of the backlog of knowledge which our training has given us, but to use it only after we have proven its application to our own particular problem. Second, any definite bit of research takes time and involves two selling processes to management: (1) the selling for cooperation in trying the experiment and (2) selling the results, so that they will be used. We have become so used to talking of correlations, standard deviations, and so forth, that we sometimes forget that to the layman these terms mean nothing. It takes imagination to restate our findings, but it pays.

Finally, the company's industrial psychologist may solve problems by application of his own knowledge and that gained from published material. He may call in a specialist for temporary help, or he may advocate the employment of a specialist when need seems to demand it. All have their place.

To sum it all up, we are general practitioners in the field of psychology.

SECTION OF PHYSICS AND CHEMISTRY

THE ROLE OF IONIC DEFORMATION IN SURFACE CHEMISTRY*

By W. A. WEYL†

Surfaces and interfaces are the seat of asymmetrical arrangements of ions. In these strong electrical fields, one has to expect that ions change their electron density distribution, which, in turn, must cause them to assume chemical properties which differ from those of the same ions in the symmetrical environment of their aqueous solutions. The polarized or deformed ion has an electron density probability distribution which has a lower symmetry than that of the gaseous or of the hydrated ion. One can express its state of polarization in terms of the character of the binding forces as well as in terms of the formation of a dipole or of a multipole. These descriptions can be useful in many ways, but they do not lend themselves directly to a chemical interpretation.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, May 10, 1950.

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This paper represents an attempt to describe an adsorbed, strongly deformed ion in terms which indicate that its chemical behavior has assumed directional properties.

A cupric ion is polarized when adsorbed at a metal. The polarizability of an ion represents its ability to adjust its electron distribution probability and, with it, its own force field in order to suit best the force field of the environment. One may expect, therefore, that the deformation of the electron cloud of the Cu^{2+} ion is such as to lead towards the establishing of a "metallic bond" in the direction of the metallic phase and towards increasing the strength of its "ionic bond" in the direction of the ionic solution. This type of deformation can also be described as an inner disproportionation of the Cu^{2+} ion which, in its extreme, would cause one-half of the Cu^{2+} ion to become metallic copper (higher electron density than that of the Cu^{2+} ion). The other half, having lower than average electron density, would then correspond to a copper ion with a fourfold positive charge. This description explains the greater tendency of the deformed ion to attract electrons from an electron donor. The combination metallic copper-cupric oxide is reduced more readily by hydrogen than the pure oxide, free of copper. Since the reaction leads to metallic copper and thus is producing its own catalyst, however, the reduction of CuO has an induction period and is autocatalytic.

Similar descriptions of the state of polarization of adsorbed ions can be given for carrier surfaces other than metals. An attempt is made to present a few simple rules which describe the shift in chemical behavior if an ion is transferred from its aqueous solution to an interface. Catalytic phenomena explained on this basis can be easily correlated with physical phenomena such as wettability and hygroscopicity of solids, semiconductor properties of thin metal films, or the increased electron emission of the oxide coated cathode.

Introduction. The parameters used for describing a crystal as an infinitely extending network—namely, symmetry, coordination number, and internuclear distance or bond length—are not valid for the crystal surface. The coordination of surface ions is necessarily incomplete and the internuclear distances are different from those of the bulk, because surface ions are exposed to one-sided attraction. In the past, the problem of the surface structure of crystals and its deviation from that of the bulk has been approached primarily from a physical-mathematical viewpoint. The early discussions of Born, Madelung, Lennard-Jones, and their associates are well known. More recently, Verwey[1] in Holland and Stranski and Moliere[2] in Germany have contributed to this problem.

It is known that, as soon as the polarizability of the ions exceeds a certain value, one has to expect the formation of a special structure in the outermost surface planes of an ionic crystal. Ions of low polarizability prefer positions of highest symmetry and lowest electrical field strength. In a NaCl crystal, for example, the Na^+ ions are symmetrically surrounded by the anions. Ions of high polarizability can be deformed, however, to such an extent that the gain in deformation energy is more than sufficient to compensate the Coulomb repulsion between ions having electric charges of the same sign. In the mercurous chloride, for example, the highly polarizable Hg^+ ions occupy positions between the anions and other Hg^+ ions. Such a crystal has a lower symmetry, and the cations are not in the positions of lowest electrical field strength but in the electrical field between Hg^+ and Cl^- ions. In a strong electrical field, the concept of ionic size loses its significance. In a highly symmetrical environment, *i.e.*, in a crystal or in a solution, it is justifiable to assign to each ion a certain "radius," because the repulsion force between two ions approaching each other increases very strongly as soon as a certain critical distance is reached. For all practical purposes, a closer approach becomes impossible and it is, therefore, justifiable to treat the ions as rigid spheres. This size of the ion loses its meaning if the ion is transferred into an asymmetrical environment, a fact which has been emphasized by Fajans[3] in his discussion of the internuclear distances of thallous compounds as compared with those of the alkalies. The identical cell dimensions of thallous perchlorate and rubidium perchlorate indicate that the Tl^+ and the Rb^+ ions have identical sizes when compared in symmetrical force fields. As a result of the one-sided attraction within the vaporized molecule, however, the internuclear distance in TlCl (2.55 A.U.) is much smaller than in RbCl (2.89 A.U.) and only a little larger than in NaCl (2.51 A.U.). In the thallous iodide, the distance (2.87 A.U.) is even smaller than in sodium iodide (2.90 A.U.). This mutual deformation of ions in asymmetrical force fields has been described also in terms of the character of the binding forces, and one says that the thallous halides are more covalent than the corresponding alkali salts.

For the purpose of interpreting the electrical properties of polarized substances, however, the degree of covalency is of minor concern, and it is preferable to describe the deformed ions as electrically distorted units having dipoles. For example, Roberts[4] used this approach in studying the electrical properties of alkali halides, and for this purpose it proved to be most helpful.

The deformation of ions in the outermost surface plane can be expressed in terms of dipole or multipole formation, as well as in terms of the character of the binding forces. In the last few years, we have made laboratory examinations of deformation of the ions at crystal surfaces by means of their characteristic chemical reactions. For this purpose, the rules derived by Fajans[5] were applied, and it was found advantageous to describe a polarized or a deformed ion in terms which indicate that, with its deformation, not only its electron density distribution, but also its chemical properties, have become asymmetrical and directional.

Since the development of modern crystal chemistry, through the work of Goldschmidt,[6] it has been known that the role which an ion plays in a crystal structure is determined by its size, charge, and polarization properties. Later workers in this field, however, overemphasized the size and the charge because these parameters lend themselves to an easy numerical description. As a result, the polarizability of an ion, one of its major properties, has not been sufficiently considered.

At the present time, Frankenburg, Komarewsky, and Rideal[7] are bringing out a series of books on "Advances in Catalysis." In the preface of the first volume they write: "a purely empirical approach is still the only safe way to search for efficient catalysts. . . ." It is true that one cannot predict quantitatively the catalytic activity of a substance, but it will be shown in this paper that there is a fair chance to understand and even to predict catalytic phenomena, at least in a qualitative fashion.

In the same preface, these authors write: "In other words, a science of catalysis has to be erected on foundations which still have to be laid." It seems, however, that the foundations for the science of catalysis are very clearly defined.

Heterogeneous catalysis is "chemistry in asymmetrical force fields" and, as such, it can be understood if one knows how the electrical field affects the chemical properties of an ion. It is the polarizability which describes the response of ions to electrical fields. This property has been neglected for too long.

It is the object of this paper to discuss surface properties of two extreme groups:

one having cations of the non-noble gas-type with very high polarizability, such as Hg^{2+}, Pb^{2+}, *etc.* and the other having small cations of the noble gas-type with low polarizability, such as Si^{4+}. The proton having no electrons has zero polarizability and, consequently, H_2O represents an extreme case of the second group.

Polarization Properties of Ions. Our knowledge of the polarization properties of ions is based primarily on the interaction between their electron clouds and the alternating electrical field of light. The response of the outer, more loosely bound electrons is by far the most important contribution to this interaction, so that the optical properties of an ion or molecule, in particular their refractivities, are closely related to their chemical properties.

The concept that the outer electrons of an atom or ion do not move in rigid orbits, but are affected by their immediate environment, goes back to Haber, Reis, Langmuir, Lewis, and others. A systematic treatment of polarization phenomena and the inter-relation which exists between polarization properties and physical or crystal-chemical properties can be found in the publications of Fajans and his students, Grimm and Sommerfeld, as well as Goldschmidt. Especially, Fajans[5] and his students evaluated the deviations from additivity of the polarizabilities in respect to the interaction of ions when forming compounds or crystals. These relations are often referred to as "Fajans' Rules," and the most pertinent ones will be mentioned here.

(1) An anion is larger than a cation with the same number of electrons. With increasing positive charge, a nucleus exerts stronger attraction forces upon its electron cloud. As a result of this "tightening" of the electrons, their response to the electromagnetic field of light decreases. The polarizability decreases in the following order:

$$O^{2-} > F^- > Ne > Na^+ > Mg^{2+} > Al^{3+} > Si^{4+}.$$

(2) A similar tightening of the electrons of an anion can be observed if the anion combines with protons. The polarizability of Cl^- ions is much greater than that of HCl. The polarizability decreases in the following series:

$$O^{2-} > OH^- > H_2O > H_3O^+.$$

(3) A tightening of the electrons of an anion is also noticeable if it is brought into the positive force field of cations. This effect increases (a) with the size (polarizability) of the anion, for example, in the following order:

$$F^- < Cl^- < Br^- < I^- \text{ or } O^{2-} < S^{2-} < Se^{2-};$$

(b) with decreasing size of the cation—the polarizing power of cations increases in the order:

$$Cs^+ < Rb^+ < K^+ < Na^+ < Li^+ \text{ or } Ba^{2+} < Sr^{2+} < Ca^{2+} < Mg^{2+};$$

and (c) with increasing positive charge of the cation

$$Mg^{2+} < Al^{3+} < Si^{4+} < P^{5+}.$$

(4) Anions exert a similar, but in most cases a weaker, influence upon the cations. Cations having the same electronic structures are smaller and they are less polarizable, because their corresponding orbits are much closer to their nuclei and are, therefore, already tightened by their positive force fields.

(5) The mutual deformation of ions is at a minimum for ions of the noble gas-type. It is greater for ions having a complete 18 shell and reaches its highest value for ions with incomplete outer orbitals. The polarizing power and the polarizability increase from Sr^{2+} (8) to Cd^{2+} (18) and are greatest for Pb^{2+} (18 + 2).

(6) The mutual deformation is at a minimum in crystals of high symmetry. It reaches its maximum in the asymmetrical force field of the vaporized molecule or in the surface.

This last statement concerning the polarization or deformation of ions in asymmetrical force fields makes it imperative to the chemist interested in heterogeneous catalysis to pay attention to the polarization properties of ions. It is true that one can go relatively far in the understanding of crystals and in the interpretation of their properties without using the concept of polarization if one restricts oneself to (a) crystals of high symmetry; (b) crystals consisting of noble gas-type ions of low charge and low polarizing power; and (c) properties which do not involve the temporary formation of asymmetrical force fields, for example, compressibility, but not plasticity; solubility, but not wettability.

Pauling[8] derived a number of basic rules governing the atomic structures of complex ionic crystals consisting of polyhedra. These rules were very wisely limited to ions having a certain size and charge and a complete octet configuration (noble gas-type ions), because this restriction excludes major polarization effects.

TABLE 1 lists the molar refraction of gaseous ions in cc. according to Bauer and Fajans.[5]

TABLE 1

		He	*Li^+*	*Be^{2+}*
		0.521	0.08	
O^{2-}	*F^-*	*Ne*	*Na^+*	*Mg^{2+}*
6.95	2.44	1.00	0.47	0.26
S^{2-}	*Cl^-*	*A*	*K^+*	*Ca^{2+}*
22.6	9.06	4.20	2 25	1.40
Te^{2-}	*I^-*	*X_2*	*Cs^-*	*Ba^{2+}*
40.9	19.21	10.43	6.53	4.73

In TABLE 2, the polarizabilities of some ions (in cubic angstroms) are listed, as

TABLE 2

Li^+	11.7	F^-	25.8
Na^+	20.2	Cl^-	62.8
K^+	42.3	Br^-	81.0
Rb^+	58.1	I^-	109.4
Cs^+	71.3	O^{2-}	30.0
Mg^{2+}	11.4	Cu^+	27.2
Ca^{2+}	34.9	Ag^+	39.4
Sr^{2+}	49.1	Tl^+	90.1
Ba^{2+}	70.6		

taken from the literature by Roberts.[4] Both tables are interesting because they show the connection between polarizability and molar refraction. For the following discussion, however, it is not necessary to use the quantitative data available on this subject. It can be shown that the use of Fajans' rules on the mutual polarization of ions allows us to explain and to predict catalytic phenomena in a fairly satisfactory way.

A Derivation of Some Simple Rules Describing the Change in Chemical Properties of Adsorbed Ions of High Polarizability. Polarizability and atomic or molecular refractivity are proportional and, therefore, it has become customary to express the polarizability of an ion by its refractivity (TABLE 1). The response of an electronic system, however, is not restricted to alternating electrical fields of high frequency, but is just as noticeable in a static electrical field. In atomic dimensions, the ions exert forces upon each other which correspond to electrical fields of several million volts per cm. In the interior of a NaCl crystal, a Na^+ ion finds itself in a symmetrical surrounding and, therefore, in the weak electrical force field between Cl^- ions. This is no longer true for a Na^+ ion in the crystal surface, but the latter is in a strong electrical field with the Cl^- ion on one side and no such anion at the other side to balance it.

For the understanding of surface chemistry, one has to realize that the polarization of a surface ion exposed to one-sided attraction has changed its chemical properties. For explaining catalytic actions, it is not sufficient to express deformation or polarization as the induction of a dipole or multipole, because this terminology has no obvious connection with the chemical properties of the ion. The change in chemical properties can be described only in a qualitative fashion, because we still lack the terminology for describing an electron configuration which corresponds to an intermediate state between two stable quantum configurations. We have, for example, names for Cr^{2+} (chromous ion) and for Cr^{3+} (chromic ion) but not for a unit which on one side has an electron density somewhere between these two configurations. As a result, the "chemical description" of the polarized ion is imperfect; it remains qualitative in character and must be considered only a first approximation to the actual conditions. Nevertheless, its usefulness in explaining surface chemical properties and in predicting the catalytic behavior of ions should make its presentation worthwhile as a first step in laying the foundation for the science of catalysis.*

Scheme I† shows a Ni^{2+} ion adsorbed at the surface of a noble metal, for example, platinum. A metal represents a highly polarizable medium because of the extreme mobility of its "free electrons." It can easily adjust its own force fields to suit the requirements of its environment. The approach of the Ni^{2+} ion leads to the formation of a "metallic bond" between the Ni^{2+} and the platinum. The nickel ion is polarized to such an extent that it can now be described as consisting partly of atomic nickel and partly of Ni^{4+}. That portion of the nickel ion which is extended toward the solution assumes properties which approach those of nickel in a higher state of valency. As mentioned before, such a description is qualitative in character and one should not take it too literally, but it gives us the right direction in which the properties of a polarizable cation are changed when it is adsorbed at the surface of a noble metal. This change may also be described as an "inner disproportionation."

> *Rule I.* Cations of high polarizability, adsorbed at the surface of a metal, extend metallic bonds towards the metal surface and, as a result of this deformation, their chemical behavior towards the solution resembles that of the same element in a higher state of oxidation.

This description of the polarized cation, in chemical terminology, makes it possible for us to understand and even to predict catalytic phenomena involving noble metals as catalysts.

The development of the latent photographic image is one of the best known examples of a catalytic reaction covered by Rule I. The developing bath contains a reducing agent which is not sufficiently strong to reduce the undeformed silver ions but which can react with those that are deformed by their contact with a noble metal. The catalyst, the silver speck, is the result of photolysis of the silver bromide. The asymmetrical force field induced by the sensitivity speck changes the random distribution of the silver atoms into an agglommeration near the surface. As soon as the number of atoms is sufficient to impart metallic properties to this agglommeration, it becomes a catalyst. Silver ions in contact with this metal speck are polarized in the fashion described above and are then able to accept electrons from the developer solution. The silver ions either may come from the bromide grain (chemical development) or they may be added with the developing solution (physical development). This lowering of the activation energy of the reduction of heavy metal ions by deforming them at a metal-liquid or metal-gas interface is rather widely used.

An ammoniacal solution of a cupric compound does not react with formaldehyde except in the presence of a noble metal, such as Ag, Au, or Pt, where metallic copper may be deposited. Vomer[9] has taken advantage of the catalytic activity of metallic silver on this reaction and has developed a method for depositing copper films on glass. For this purpose, the glass is first coated with a very thin layer of metallic silver and then brought into contact with the ammoniacal copper-tartrate-formaldehyde solution. The silver layer can be so thin that it is invisible, and still it causes the Cu^{2+} ions to be reduced to the metal. According to Rule I, we may describe this catalytic reaction as follows: The metallic film of silver adsorbs Cu^{2+} ions and deforms them in a fashion described by scheme I. A metallic bond is formed between the Ag metal and Cu^{2+} ions by increasing the electron density between the metallic phase and the Cu^{2+} nucleus. This, in turn, causes the cupric ion to resemble a Cu^{3+} or a Cu^{4+} ion towards the solution. It is not surprising that such a deformed cupric ion now attracts electrons from the reducing agent and changes into a neutral atom.

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* Work on this subject is being sponsored by the Material Branch of the Office of Naval Research.

† Lantern slides were shown at this point.

This atom then polarizes another Cu^{2+} ion in the same fashion until the reduction of the cupric salt is completed or the reducing agent is oxidized.

The same explanation applies to a method of precipitating metallic nickel on glass which has been developed by Pragst.[10] An ammoniacal solution of a nickel salt containing NaOH does not readily react with hydrazine compounds except in the presence of noble metals which catalyze the reduction of Ni^{2+} to the metal or the oxidation of hydrazine to nitrogen by Ni^{2+} ions.

The surface of a solid containing cations of low polarizability is formed primarily by anions. The higher polarizability of the anions allows them to adjust their electron configurations better and, with them, to decrease their force fields extending into space. This deformation lowers the surface free energy of the solid. Scheme II* represents the adsorption of a silver ion on the surface of alumina, *i.e.*, a surface which consists primarily of anions. The deformation of the Ag^+ ion is such that the electrons are repelled by the carrier so that the Ag^+ ion behaves like an Ag^{2+} ion in the direction towards the carrier. Its asymmetrical electron distribution causes it to assume chemical properties resembling atomic silver in the direction of the solution.

> *Rule II.* Carriers consisting of anions having a low or a medium polarizability and of highly charged cations deform adsorbed cations of the non-noble gas-type in a manner which causes their chemical behavior to be shifted toward that of the same element in a lower state of oxidation ($Ag^+ \rightarrow Ag^\circ$).

This description of the polarized cation will be used to explain some catalytic reactions.

The deposition of a silver mirror on glass is a typical catalytic reaction covered by Rule II. Gardner and Case[11] write: "In commercial silvering many manipulators follow the cleansing of the glass surface with nitric acid by a vigorous swabbing with a saturated solution of stannous chloride ($SnCl_2$) which is carefully rinsed off with warm water. This is regarded as an essential feature in many of the 'secret processes' used in the trade." The adsorbed Sn^{2+} ion which is deformed under the influence of the OH^- or O^{2-} ions of the glass surface can be described as an Sn^{4+} ion in its behavior toward the glass and as metallic Sn in its reactions toward the silvering solution. There are many different processes for depositing silver on glass, but all of them have one thing in common: a mixture of a silver compound with a reducing agent which does not react with noticeable speed but requires a catalyst. The presence of deformed Sn^{2+} ions at such a surface catalyzes the Ag deposition and provides good adhesion according to the scheme:

glass surface — (Sn^{4+} — Sn°) — metallic silver.

Between the polarized Sn^{2+} ion and the glass, the strong Coulomb forces characteristic for O^{2-} — Sn^{4+} ions are in effect. Between the polarized Sn^{2+} ion and the metallic silver, one can expect the formation of metallic bonds.

The reduction of Ag^+ ions is catalyzed by the glass alone because the Ag^+ ions can undergo a similar deformation as described for the Sn^{2+} ions. This can be proved by the "metallic" properties of Ag^+ ions adsorbed at a carrier such as glass or Al_2O_3.

The decomposition of hydrogen peroxide is not affected by glass powder or by alumina. The presence of Ag^+ ion in aqueous solution is equally ineffective on the rate of this reaction. This reaction can be strongly accelerated by noble metals such as silver, probably because the surface of a metal, with its high electron density, increases the probability of the electron transfer which leads to O_2 and H_2O according to

$$2H_2O_2 = 2H_2O + O_2.$$

We have found[12] that by adsorbing silver ions at glass or at alumina one can imitate the surface of a metal. The simultaneous presence of Ag^+ ions and glass powder or alumina catalyzes this reaction. The same is true for Hg^{2+} ions which are ineffective by themselves but turn into positive catalysts when adsorbed at alumina.

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* Also illustrated by a lantern slide.

The fact that a carrier influences the chemical properties of an adsorbed ion or molecule is so well established that a chemist takes for granted that the chemical behavior of an adsorbed substance depends on the nature of the carrier. Newton's principle of action and reaction being equal allows us to expect that the behavior of the carrier should also be affected by the nature of adsorbed ions.

Due to their free electrons, noble metals show a number of characteristic catalytic activities such as enhancing the oxidation of hydrogen gas or the decomposition of H_2O_2 molecules in aqueous solutions. The adsorption of ions at the surface of a metal can be described as the result of its producing image forces in the highly polarizable metal surface. The contact of a metal surface with the cube face of a NaCl crystal, for example, produces in the metal a checkered pattern of electrical fields characterized by higher and lower electron densities. The Na^+ ions attract, and the Cl^- ions repel, electrons. As a result, the metal surface no longer permits electrons to flow "freely." In the surface of the polarized metal, the flow of electrons now requires an activation energy, because they have to move through areas with higher electron density from which they are repelled and through areas of lower electron density which have a tendency to retain them.

> *Rule III.* A metal in contact with crystal surfaces or with adsorbed dipole molecules which produce positive and negative force fields has its electronic conductivity changed from that of a true conductor toward that of a semiconductor. The electrical resistivity increases in magnitude and its temperature coefficient decreases. For very thin metal films, the temperature coefficient of the surface resistivity may become negative.

This phenomenon has been observed for thin films of metals evaporated *in vacuo* onto a degassed glass surface. Appleyard[13] was the first to report this unusual behavior of alkali metals and emphasized that this phenomenon cannot be explained by any simple theory of metallic conduction: "It is indeed clear, that no form of mere geometrical disintegration of the film such as spongyness or porosity can give a negative coefficient (of electrical resistivity) to a film if the bulk metal has the normal positive temperature coefficient." The same phenomenon has been found for thin platinum films on glass by Vodar and Mostowitch.[14]

Von Itterbeek and de Greve[15] recently called attention to the negative temperature coefficient for electrical resistance of nickel films having a thickness below 40 mμ. The same authors found that the electrical conductivity of thin nickel films may deviate from Ohm's law and increase with the applied voltage.

For metal films which are sufficiently thin, the normal electrical properties of the bulk metal are decreased at the expense of the semiconducting properties of the surface films. The surface film of a metal shows all the characteristics of a crystal with "available" but not "completely free" conduction electrons: negative temperature coefficient for electrical resistances and deviation from Ohms Law.

This polarization of a finely subdivided metal, either as film or powder, offers unusual possibilities for synthesizing semiconductors. Recent work of Hausner[16] seems to be a step in this direction from a commercial point of view.

The magnitude of the force field of the ionic substance in contact with the metal is very important for this purpose. Compounds such as rubidium iodide cannot be very effective in this respect because the high polarizabilities of the large Rb^+ and I^- ions result in weak force fields (low surface tension). MgO or BeO, in contact with a metal, should be much better suited for producing semiconductors. The difference in the polarizabilities of a cation and an anion decides which ion is the most likely to be adsorbed at a metal surface. The greater the polarizability of an ion, the better it is suited to bridge the gap between two incompatible media, *i.e.*, to lower the interfacial energy of systems such as metal-gas or metal-aqueous solution. This phenomenon is very important for reactions which are catalyzed by a noble metal, because it shows how the activity of a metal can be poisoned as well as enhanced. Bringing cations of low polarizability (noble gas-type ions) onto the surface of a noble metal accentuates its "metallic properties" because their positive charge raises the electron density in the metal surface. Cations with a complete octet shell have long been used to produce "super metallic" properties in metals. Electronic devices take advantage of the fact that the oxide-coated cathode has a much greater electron emissivity than a pure metal at the same temperature because the polar oxide induces spots of high electronic densities in the polarizable metal as described by Weyl and Forland.[17] The same applies to the catalytic properties of noble metals. As early as 1844, Dobereiner[18] improved one of the first catalysts ever investigated, namely, the platinum sponge, by bringing alkali onto its surface. In the same fashion, Bredig[19] increased the catalytic activity of this metal in respect to the decomposition of hydrogen peroxide by the addition of alkali.

> *Rule IV.* The catalytic activity of a metal surface can be accentuated by bringing cations with medium or low polarizability onto its surface. Their positive force fields increase the electronic density in the metal surface and cause the surface to assume "super metallic" properties.

For this purpose, cations of low to medium polarizability are best suited, because cations of high polarizability change the character of the metal surface completely. A metal surface covered with Pb^{2+} ions or with Hg^{2+} ions has lost its catalytic properties because these cations are so strongly deformed by the metal that the system Metal-Pb^{2+} can be adequately described by the scheme Metal — (Pb° — Pb^{4+}). As a result, the metallic character disappears and strong ionic forces are extended into the solution or into the gas phase. Chemical reactions which are catalyzed by a surface of high electron density are not likely to occur at a noble metal covered with Hg^{2+} or Pb^{2+} ions. Cations of high polarizability act as catalytic poisons. The same effect can be produced by compounds containing large and, therefore, highly polarizable anions such as S^{2-}, Se^{2-}, P^{3-}, N^{3-}, *etc.*

> *Rule V.* Those catalytic activities of noble metals which are based on the high electron density and electron mobility characteristic for their surfaces are poisoned by cations or anions of high polarizability (Pb^{2+}, Hg^{2+}, S^{2-}, P^{3-}, N^{3-}).

The greater the deformability of ions, the better they can bridge the gap which exists between a metal and a gas or a metal and a solution. These ions lower the free energy of the interface, and for this reason they are preferentially adsorbed and are effective even in very low concentrations.

Just as Pb^{2+} ions can be made to assume the character of Pb^{4+} ions towards the solution by adsorbing them at a metal surface, so also can one polarize them in the opposite direction so that they will assume "metallic properties" towards the solution. This is the case for the surface of a PbF_2 crystal, where the Pb^{2+} ions are deformed by the F^- ions.

The polarizabilities of Pb^{2+} and Br^- ions are both rather high, so that one cannot expect the surface of a $PbBr_2$ crystal to differ greatly from that of the bulk. Going from lead bromide or chloride to the fluoride, one can expect that the surfaces are formed predominantly from Pb^{2+} ions. The surface free energy of a PbF_2 crystal is lowest if the most polarizable ion, in this case no doubt the Pb^{2+} ion, forms the extreme outer surface layer. The deformed Pb^{2+} ions can be described as extending strong positive force fields towards the fluorine ions (thus behaving like Pb^{4+} ions toward the interior of the crystal). The fluorine ions, on the other hand, repel the two outer electrons of the Pb^{2+} ions in a fashion which can be described as causing these ions to assume the high electron density of the atoms in the outward direction. This reasoning led to the discovery of PbF_2 behaving like a noble metal and catalyzing the decomposition of hydrogen peroxide.[20]

Frequently, the conditions described in these rules may not apply to a reaction from its very beginning, but develop as the reaction proceeds. A typical autocatalytic reaction, *i.e.*, a reaction where one of the reaction products assumes the role of the catalyst, is the reduction of cupric oxide by hydrogen. This reaction will be used to demonstrate the mechanism of autocatalysis and to explain why the presence of elemental copper decreases the activation energy of the reduction of its oxide.

Pease and Taylor[21] found that the induction period of this reaction can be shortened by adding metallic copper to the oxide. The reaction

$$Cu^{2+}O^{2-} + H_2 = Cu + H_2{}^{+}O^{2-}$$

can be described as the Cu^{2+} ions attracting two electrons from a chemisorbed hydrogen molecule, thus forming two protons and a copper atom. In Rule I, the combination Metal-Polarizable Cation has been described as Me — ($Cu^{\circ}Cu^{4+}$). The cupric ion, in contact with metallic copper, is deformed in a fashion which causes it to extend metallic bonds to the metal atom and, as a result, to emanate a stronger positive force field into the gas phase. Consequently, the couple copper-cupric oxide has a greater affinity for the electrons of the hydrogen and can be more readily reduced than the pure cupric oxide alone. This concept tells us why this reaction is autocatalytic and, furthermore, allows one to predict that intimate mixtures of CuO with metallic Au, Ag, or Pt will be more readily reduced by hydrogen than the pure oxide.

Combinations of this type are used for oxidizing carbon monoxide at low temperatures. Silver oxide is readily reduced by carbon monoxide and forms metallic silver and carbon dioxide. The oxides of Mn, Cu, Co, and Ni, when applied in their pure states, do not react with carbon monoxide at room temperature with sufficient speed, but they will do so when mixed with Ag_2O. Very probably, this mixed catalyst (Hopcalite) owes its activity to the formation of atomic silver, which aggregates to form tiny silver specks analogous to the formation of the latent image in the photographic process. A speck consisting of ten or twenty Ag atoms polarizes the cations of the other metal oxides as described above. This catalyst resembles the photographic emulsion, which becomes developable by a weak reducing agent once a silver speck has formed.

Disturbance of the Surface and its Depth Action. It is not possible in this paper to give a complete description of the influence of the polarizability of ions on the surface chemistry of solids. In recent publications, the author described in more detail its effect on the hygroscopicity[22] and the wettability[23] of solids, as well as on the adhesion forces between different solids.[24]

There is one important feature, however, which must be mentioned briefly, because it seems that this particular manifestation of the polarizability of the lattice constituents is not yet generally understood.

The fact that a surface of a solid differs from the bulk is obvious and is well recognized. If one assumes a different surface structure, however, one must also assume a finite distance between this structure and that of the bulk. Bulk structure and surface structure cannot be treated as independent of each other and, consequently, a depth action results which depends upon the polarizability of the constituents.

This depth action might be negligible in substances of low polarizability (NaCl), but it must be considerable in metals. W. Braunbeck[25] examined the conductivity of a platinum foil 2 microns thick in different gases. The abrupt transition from metal to vacuum creates a disturbance of the surface which must exert a considerable depth action, *i.e.*, it causes the atomic structure of the metal to deviate from its ideal or bulk structure over a region which is many atoms thick. By bringing gases onto the metal surface, the abruptness of the transition, metal-vacuum, is decreased by the adsorbed gas molecules, and the surface film approaches a more orderly arrangement of the atoms, as can be seen from the electric conductivity. Helium, a gas of low polarizability, was found to increase the conductivity of these platinum foils 0.1 per cent over that *in vacuo*. The more polarizable argon and oxygen atoms were found to be three and five times respectively as effective as helium.

The depth action which a disturbance creates in highly polarizable metals can be judged from the fact that it is possible to harden metallic lead by a minor addition of the still softer sodium or calcium metal. Foreign atoms of different size, charge, or electron configuration produce disturbances in the metal which must have considerable depth action in order to be effective in these low concentrations. For chemists interested in catalysis, there are two manifestations of the depth action in metals which are of utmost importance.

The first phenomenon deals with the metallic properties of a noble metal as a function of the particle size. Good electronic conductivity of the surface of a metal requires a certain state of order. In order to have such a surface, it has to be backed up by a deep layer of the metal. This means that the catalytic properties of a noble metal can decrease if its subdivision is carried too far. It has been found, for example, that the catalytic action of a certain quantity of metallic gold first increases with increased surface development, but passes through an optimum in order to decline with further subdivision.

Some data of Rusznyak[26] will be given as an example. The decomposition of 50 per cent of the hydrogen peroxide added to colloidal gold required 14 minutes for a sol having a particle size of 42 $m\mu$. Decreasing the particle size to 31 $m\mu$ more than doubled the time, and particles of 29 $m\mu$ required 32 minutes. The finer particles are less efficient because their surface structure is less orderly and less conducive to electron transfer than that of a large gold particle. It is well established that a silver speck must have a certain size in order to become a catalyst and to make the silver bromide grain developable.

The strong association between the electronic properties of finely divided metals and the surrounding media is not limited to conductivity (Braunbeck[25]) but manifests itself in the light absorption as well. Nearly one hundred years ago, in 1857, Faraday[27] published his classical studies on the relations of gold and other metals to light and described that the color of finely subdivided metal depends on the nature of the environment: "The effect strengthens in my mind the thought that in the ruby fluids the deposited particles are frequently associates of water and gold. It is a striking case of the joint effect of the media and the gold in this action on the rays of light and the most striking amongst those where the medium may be changed to and fro."

The second manifestation of the depth action is related to the fact that the properties of a metal surface can be changed by backing it up by various carrier substances. The catalytic activity of cobalt or of nickel films can be improved by bringing these metals onto the surface of certain carriers, such as ThO_2. Just as the electron emissivity and the catalytic activity of a metal surface can be improved by bringing certain cations onto the surface (Rule IV), its catalytic properties can be modified by backing the film up with different oxides. One of the most spectacular effects revealing this depth action is an observation made by Schwab[28] and his collaborators.

They found that copper or nickel precipitated onto the surface of dextro-rotatory quartz crystals dehydrates laevo-rotatory butyl alcohol faster than its optical antipode. Normally, these metals, being optically inactive themselves, must catalyze both reactions in precisely the same fashion. The optically active quartz surface seems to impress its force field upon the metal film, however, and the resulting image forces cause a distortion within the metal film which has sufficient depth action to become noticeable on its other side in the form of a different geometry and different lattice distances.

Conclusions. Our knowledge of catalytic phenomena is increasing very rapidly. There is no doubt that crystal chemistry is the natural basis for the understanding of heterogeneous catalysis involving crystal surfaces. Up to the present time, crystal chemistry has been restricted to the treatment of crystals as arrays of ions which extend infinitely in space. Some of the most interesting physical properties (strength, low temperature, electronic conductivity, fluorescence, *etc.*) are governed by the deviations from ideal patterns or by the defects occurring in the crystal. The chemical properties are functions of the surface structure which, too, must be treated as a defective crystal. The parameters used for describing the bulk of the crystal cannot be applied to its surface.

In order to understand better certain color phenomena concerning natural minerals and synthetic pigments, it became necessary to extend crystal chemistry so as to include defective structures. This could be done on the basis which was laid by Goldschmidt, but the emphasis had to be shifted towards ionic deformation. Goldschmidt, in his work on the properties of crystals as related to their chemical composition, treats the size, charge, and polarizability of the ions on a par. This treatment is justified as long as it refers to certain bulk properties. The so-called structure sensitive physical properties and the surface chemistry, however, are most strongly influenced by the polarizability of the ions. In perfect crystals, the size of the ions is a very important property, because it determines their Coulomb forces. On the basis of Fajans' rules, Weyl and Forland extended crystal chemistry to include defective structures and explained some of the unusual color phenomena characteristic for minerals.[*]

The same ideas have now been used for explaining the chemical behavior of ions adsorbed at various carriers. As far as the qualitative aspects are concerned, this treatment is very simple. There are major difficulties concerning its quantitative treatment, however, and it will probably take a long time to bring this approach to a more quantitative basis.

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SECTION OF MATHEMATICS AND ENGINEERING

ELECTROMAGNETIC COMPUTING DEVICES*

By DONALD HERR†

This paper deals with the generalized transfer-functions available through the use of electromagnetic computing devices which consist of two concentric, right circular cylindrical, magnetic structures, each containing generalized conductor distributions, numbers of poles, skewnesses, and physical dimensions. Such devices, when used as voltage- (or other) transfer devices, in terms of the angle of relative rotation between the two magnetic structures, find great use in high-precision, continuous computing applications in analogue computers and control systems. For example, such electromagnetic induction devices may be used to provide output voltages which are proportional to the sine or cosine of the angle of relative rotation, in which case they are called resolvers; for developing output voltages which are linearly proportional to the angle of rotation itself, in which case they are called linear potentiometers; or, for the generation of other useful, arbitrary output-voltage functions of the angle of rotation.

One such specific, but by no means limiting, example is the electromagnetic induction resolver. Prior to World War II, the computing operations of vector resolution and composition, the solution of plane and spherical triangles, and the transmission and reception of position data between remote points were performed by mechanical resolvers almost entirely. These mechanical devices were essentially "Scotch-yoke" mechanisms and were large in volume, heavy, precision-machined, expensive, and seriously subject to the effects of mechanical shock and vibration. Since that time various embodiments of electromagnetic induction resolvers have appeared. One of the specific results of the research, development, design, and manufacturing program which has made this paper possible is the creation of a new high-precision, universal-frequency resolver weighing only approximately one pound, having a volume less than 18 cubic inches and a functional error less than ±0.05 of 1 per cent over 360 degrees of rotation. In addition, it possesses greatly reduced noise content over a wide range of excitation voltage and, when used with its appropriate compensating amplifier, it operates on any carrier frequency, from 50 cycles per second to greater than 2 kilocycles per second.

Essentially, the fundamental problem has to do with the control of the amplitudes of the space harmonics of the Fourier Series in describing, in most general terms, the arbitrary space (a.c.) flux-density function which it is desired to generate in the magnetic (air) gap between stator and rotor structures by the generating winding. The generating winding may be situated on either the stator or rotor structure. For purposes of dissertation, it is assumed to be on the stator structure. The problem also has to do with control of the amplitudes of the space harmonics of the angle of relative rotation between stator and rotor structures, which are induced in the output, or rotor winding. The fundamental aspect of the problem of voltage-transfer-function devices is to control thereby the amplitudes of the space harmonics of the output voltage as a function of angle to the prescribed Fourier coefficients of the arbitrary function desired. This control is accomplished ideally through the proper choice of the number of stator poles, in the slots between which the proper conductor distribution of the generating stator windings are located, by appropriate skewness of the stator slots, by a proper choice of the number of rotor poles, by appropriate conductor distribution in the rotor slots between these poles, and by choice of the proper skewness of the rotor poles.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, May 19, 1950. This was the inaugural meeting of the Section. This is a condensation and does not contain all material presented or available.

† Reeves Instrument Corporation, New York, N. Y.

For the type of symmetry of interest and for a fundamental space period of 360 degrees of rotation (one revolution between stator and rotor), the numbers of the stator and rotor poles are restricted to those which are an integral multiple of four or an integral multiple of four plus two. On the same basis, it is ideally possible to eliminate all even-order space harmonics from consideration, *ab initio*. Hence, it is necessary to deal only with the amplitudes of all odd-order space harmonics.

In the case of a resolver, the basic requirement is that the rotor output voltage consist only of the fundamental space-harmonic of the angle of rotation between stator and rotor, the fundamental period being one full revolution. Hence, the conditions are that all of the possible odd-order harmonics greater than the first to be eliminated ideally by the foregoing means. Other requirements exist, of course, such as the engineering requirements that the fundamental space-harmonic output voltage which does appear be proportional to and approximately equal in maximum value to the generating winding excitation, and that the ratio of reactance to resistance of the generating stator winding be close to the physical maximum.

It is shown for each of the two possible numbers of poles (slots), on stator and rotor, that there exist two different possible conductor distributions. Hence, four basic pole and conductor distributions exist. These are denoted as types A, B, C, and D, types C and D referring to the two possible arrays for magnetic structures with number of poles (slots) equal to an integral multiple of four.

For a double winding stator, namely, two distributed stator windings located on the same stator structure but with axis 90 mechanical degrees apart in space, if one of them is a type A, then the other is a type B; if one of them is a type C, then the other is a type C; and if one of them is type D, then the other is type D.

From the standpoint of complete suppression of unwanted space harmonics, the most superior suppression patterns are obtainable with types A and D stator windings. Type B, the necessary mate of type A, is considerably inferior to type A, whereas type C is inferior in its space-harmonic suppression pattern to all other types. Furthermore, even accepting the inferior type B winding, the use of a type A-type B pair means two different stator winding problems and two different stator winding impedances, as well as two different space-harmonic rejection patterns.

On the rotor, the same four types of pole-and-conductor arrays are possible. Here, however, because of one more degree of freedom, the types A and B are equal in their space-harmonic rejection patterns, as are the types C and D.

For both stator and rotor types A and D, the only space-harmonics which it is not possible to reject simultaneously and completely are those which are equal in order to an integral multiple of the number of poles (slots) plus and minus one.

While the generating structure (here denoted as the stator), as well as the rotor structure, may have its poles (slots) skewed for various beneficial results, it is readily demonstrated that skewing of the rotor slots alone one slot pitch will result in the further suppression of just those pairs of space-harmonics which it is not possible to eliminate completely by rotor pole-and-conductor array.

Ideally then, the problem consists preferentially of using type D pole-and-conductor arrays on both stator and rotor and skewing the rotor poles one slot pitch. For example, in a 20-pole type D stator, with the exception of the wanted first space-harmonic or space fundamental, the only other space harmonics which are generated in the air gap are the unwanted ones, namely, 19th and 21st, 39th and 41st, 59th and 61st, 79th and 81st, *etc.* Each of these appears with an amplitude with respect to the fundamental equal to the reciprocal of the order of the harmonic. As far as the type D rotor is concerned, with the exception of the desired first space-harmonic or space fundamental, in a 12-pole (slot) rotor it is possible to suppress entirely all other unwanted space harmonics except the 11th and 13th, 23rd and 25th, 35th and 37th, 47th and 49th, 59th and 61st, *etc.*, by means of the appropriate conductor distribution. Without regard to skewing, the amplitudes of the remaining rotor harmonics are proportional to, among other factors, the reciprocal of the order of the harmonic, with respect to the fundamental amplitude. As noted, it is possible, by use of rotor skewness, to suppress the amplitudes of these remaining unwanted space-harmonics of the rotor still further. By use of unity skewness, noted above, the ratio of these unwanted space harmonics with respect to the fundamental, in amplitude, becomes equal to the reciprocal of the square of the order of the harmonic.

Hence, in such a combination, the first unwanted harmonic which the stator generates and the rotor has induced in it consists of the pair, the 59th and 61st orders, and these bear in amplitude a ratio to the fundamental of approximately the reciprocal of 60 cubed, in the ideal situation.

Other considerations enter into the design of high-precision, universal-frequency computer components of this type. Chiefly, they involve the use of a high permeability, low core-loss, high saturation point, homogeneous, annealed magnetic material in both the stator and rotor structures; the proper choice of stator and rotor slot shapes and slot openings; the cleanness of the magnetic punchings and their care; the minimization of leakage flux; the proper choice of air gap width and the reduction of end and slot leakage flux; extreme concentricity of stator and rotor structures (mechanically, magnetically, and electrically); optimum ratio of stator and rotor lamination stack lengths to rotor radius and outside stator radius; maximum reactance-to-resistance ratio; and finally, the manner of winding the conductors into the slots.

The foregoing techniques can insure minimum noise voltages being generated in the electromagnetic devices, which are considerations apart from the space-harmonic distribution problem treated above.

With the electromagnetic computing device itself of optimum design, the compensation of the electrical phase-angle of the rotor output voltage, with respect to the stator input voltage, and the precise adjustment of the ratio of rotor output maximum amplitude to the amplitude of the stator input or excitation voltage require the use of a high-gain, feedback, booster-amplifier compensator. The feedback path may consist of either compensating networks or compensating networks plus an internally wound feedback stator winding. In the same unit, temperature and amplitude linearity compensation may be effected.

By the foregoing means, the residual unwanted time-quadrature and time-harmonic noise components are greatly reduced to less than a total of one part in a thousand of the maximum rotor output voltage, over 360 degrees relative stator-rotor rotation, over an input range which increases from 60 volts maximum at 60 cycles per second carrier frequency to greater than 300 volts maximum at 400 and 1000 cycles per second carrier frequency, in the same device. Additional practical factors involve the provisions for least weight and volume of the computing device and its booster-amplifier, extreme shock and vibration proofness, corrosion resistance, and minimum rotor inertia and friction.

Entirely subject to similar considerations are high-precision universal-frequency linear induction potentiometers and other voltage- (or other) function generators. Similarly, these units are provided with double-ended shafting as well as single-ended shafting and any combination of one or two stator windings and one or two rotor windings.

Based upon the results of the fundamental research, development, and design program outlined herein, the Reeves Instrument Corporation is now in full production and test of machine-wound high-precision, universal-frequency a.c. induction computing components of the type described to fulfill its own stringent needs and the needs of others in large quantities, with assured delivery and reasonable cost.

In conclusion, I would like to acknowledge the unstinting, faithful, and most competent work of my two chief assistants, Mr. John D. Loy, Master Instrument Machinist, and Mr. Norman R. Blumenstock, Electronics Engineer, in this program. Very special mention is made of Mr. Harry D. Belock, Executive Vice President and General Manager of the Reeves Instrument Corporation, whose insistence upon there being made available such a high quality resolver motivated much of this work. I express my deepest thanks to Miss Sophie Regan for her painstaking secretarial help and encouragement.

SECTION OF OCEANOGRAPHY AND METEOROLOGY[*]

INDUCED PRECIPITATION AND EXPERIMENTAL METEOROLOGY[†]

By VINCENT J. SCHAEFER[‡]

In considering the general aspects of "induced precipitation," there are a few basic scientific facts which must be recognized at the outset if we are to make substantial progress in evaluating the possibilities and limitations of cloud modifications produced by artificial means. There are three factors in the physical structure of the atmosphere which seem to be of primary importance in this respect. These are (1) supercooled clouds, (2) ice nuclei, and (3) the synoptic situation.

Supercooled Clouds. Supercooled clouds are of common occurrence in the natural atmosphere. In fact, it is most likely that all natural clouds supercool whenever their individual droplets cool below 0°C.

A supercooled cloud is inherently unstable. This is due to the difference in the partial vapor pressures of water and ice which reach a maximum at about $-14°$C. At all temperatures below 0°C, however, an ice crystal will grow at the expense of the nearby supercooled cloud droplets, which evaporate and condense in the solid phase.[1] The shift from water to ice causes an increase in temperature within an affected cloud. This heat of sublimation may range from a few hundredths of a degree in a thin stratus cloud to two or three degrees Centigrade in a large cumulus cloud. The speed of transformation may require ten or fifteen minutes or only a few seconds, depending on the concentration of ice nuclei. Very rapid transformations occur only when the concentration of ice nuclei is of the order of $1 \times 10^8/M^3$, a condition which has not yet been reported to exist in the natural atmosphere and one which probably never occurs in clouds except at temperatures colder than −39°C.

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[*] On May 22, 1950, the Section of Anthropology held a meeting at which Marian W. Smith read a paper entitled "Testing Anthropological Field Methods" No abstract of this paper has been received.

[†] On May 23, 1950, the Section of Oceanography and Meteorology held a symposium, presided over by A. F. Merewether of Pan-American Airlines, on "Induced Precipitation." The first paper, of which no abstract has been received, was read by Wallace E. Howell, Harvard University. The present paper was the second of four presented at the symposium.

[‡] General Electric Research Laboratory, Schenectady, New York.

It is a simple matter, however, to produce such a condition on a large scale by artificial means, using either dry ice or silver iodide.[2,3] It is this fact which encourages us to believe that experimental meteorology has an important future in certain phases of meteorology.

The persistence of supercooling thus seems to depend primarily on the temperature of the supercooled cloud and the presence or lack of a critical concentration of ice nuclei. In the absence of a sufficient concentration of ice nuclei within a cloud, it may supercool to a temperature of −39°C. This is the critical temperature at which spontaneous nucleation occurs.[4] It is now believed that except in very special cases, which are quite unlikely to occur in the natural atmosphere, supercooling will not extend beyond −39°C.

Ice Nuclei. Many observations of natural clouds have indicated the great variability in concentration of ice nuclei in the free atmosphere. It is because of this fact that large vertical thicknesses of supercooled cloud sometimes occur. In fact, it is often observed that large towering cumulus reach the −39°C isotherm with substantial quantities of the cloud still in a supercooled state. The transformation to ice crystals under such conditions is rapid and sometimes quite spectacular, although optical effects due to this transformation are generally absent, since the cloud droplets which freeze probably remain spherical. At other times, the cloud shifts over to snow at a warmer temperature. This variability in the persistence of supercooling is consistent with the physical facts, which are now fairly well understood due to recent work in Europe and this country.[2,5]

The concentration of ice nuclei in the free atmosphere has been the subject of "Project Cirrus" field observations[6] for more than two and a half years. Nearly 6500 observations have been made at the Mt. Washington Observatory as part of this program. Air passing the mountain summit at an altitude of about 6300 feet above sea level is introduced into a cold chamber, a supercooled cloud is formed at a temperature of −15°C to −18°C, and the number of ice crystals which form within the cloud are noted. An analysis of these data shows that the concentration of potential ice nuclei in the air which passes the summit of Mt. Washington varies by a factor of more than a millionfold. These experimental facts are of great importance in a consideration of what might be accomplished by the seeding of unstable clouds by artificial means.

Similar studies of natural ice nuclei now underway in this country and England substantiate the Mt. Washington studies. In addition, they disclose critical temperatures below which there seems to be a sudden increase in nuclei. In New Mexico, this temperature is about −22°C;[7] in England, it is reportedly[8] at about −32°C. This increase is of a much lower magnitude than the critical spontaneous nucleation temperature of −39°C, but it may go from $1 \times 10^2/M^3$ to $1 \times 10^6/M^3$ between −19°C and −23°C. The 10,000-fold increase in potential ice nuclei over a temperature range of 4°C is quite in accord with the characteristic activity of foreign particle nuclei which has been studied by the writer.[2,6] Many natural soils from the region of the Great Plains and the Northwest, when introduced into a cold chamber as a fine dust cloud, are quite useless as ice nuclei at −13°C but 100 percent active at −22°C. The wide range in activity which has been found between various samples of such foreign particles and the randomness in the manner whereby they become airborne particles seems to provide a logical answer to the variability and unpredictability in occurrence of supercooled clouds.

The Synoptic Situation. The general weather situation which accompanies the formation of clouds may have a profound effect on their subsequent development and possible precipitation. The presence of a strong inversion or a layer of dry air aloft limits the formation of thick cumulus clouds. The lack of sufficient moisture or a favorable lapse rate also imposes restrictive limitations on the development of clouds. Any feature of the synoptic situation which puts a restraint on the convergence of moisture laden air also exerts its effect on the development of precipitation.

When areas of unstability are present in warm, moisture laden air or when a strong flow of such air encounters heated mountain ridges or a dense mass of cold air, conditions are potentially suitable for the development of precipitation. The almost infinite variation in the synoptic situation which occurs in the atmosphere makes it difficult, however, to establish any simplified picture of such situations. Langmuir has pointed out[9] that the formation of storms is an excellent example of divergent phenomena—the development of very complicated systems from simple beginnings. It follows from this concept that the difficulties still experienced in preparing reliable forecasts of certain types of storms, especially those which suddenly burst into full vigor without prior warning, are prime examples of divergent phenomena. In some cases, even with much additional physical information than is now available, it may always be difficult, if not impossible, to predict the occurrence of such storms with any degree of accuracy.

Aside from these complicated and difficult problems, it is a fact that there are many features of clouds and cloud systems which we may hope to understand eventually.

The Economic Aspects of Cloud Modification. Due to the complex features of the atmospheric processes which produce substantial quantities of snow and rain on the ground, it is my considered opinion that it is too early to reach definite conclusions as to what can or cannot be done with respect to squeezing additional amounts of moisture out of clouds.

A very important paper related to this problem was recently presented in Washington by Braham,[10] which, on the basis of a study of some 53 thunderstorms in Florida and Ohio, showed that only about 10 per cent of the moisture flowing into a thunderstorm system reaches the ground as precipitation. This precipitation represented about 20 per cent of the condensed cloud water.

If, by careful artificial seeding techniques, a method could be devised to employ the potential energy of the synoptic situation to increase the amount of precipitation, reduce the torrential rain and violent winds, and, at the same time, prevent the formation of hail and lightning, few persons would dispute the economic value of such an operation. While it is too early to say with certainty that such results can be achieved, it seems very possible that such operations are feasible, if in fact they have not already been obtained in some of our New Mexico experiments.[11]

The formation of extensive earth-shadowing clouds by the development of "false" cirrus is another aspect of experimental meteorology which demands careful study. Langmuir has described[11] such formations in sub-tropical regions which cover thousands of square miles. The writer has made observations in New Mexico[6] concerning the ground temperature with and without such snow streamers and found the temperature 2 mm. below the surface to be 140°F on a cloud-free day and 70°F when thin cirrus streamers drifted from the top of a towering cumulus about 50 miles downwind. In this particular instance, although a large cloud formed, it not only reduced the possibility of the formation of orographic cumulus in the shadowed area but the condensed moisture in the cloud formed no precipitation either from the thin cirrus carried downwind or below the cloud responsible for this formation.

In another series of observations, our "Project Cirrus" group[12] studied cumulus and stratus clouds both off the coast and over the mountain ridges of Puerto Rico.

We found measurable amounts of persistent light rain to come from stratus clouds 300 feet thick and fairly heavy rain to fall from relatively small and warm cumulus clouds. These cloud systems seemed to be very efficient producers of precipitation.

It will be very difficult to establish with certainty the value of a cloud-seeding operation in the Northeast, such as is underway in the local New York City watershed. In general, it seems that whenever substantial cloud systems develop in this region rain occurs. Whether the clouds are efficient precipitators like those of Puerto Rico or have properties like the Florida and Ohio thunderstorms, which seem to deliver only 10 to 20 per cent of their condensed water as precipitation, it is important at this stage to evaluate these potentialities. Such factors as the source of the moisture, the concentration and nature of the condensation and sublimation nuclei, the effect of the mountainous terrain in the reservoir area, and the contact between different types of air masses—these are factors which are important and measurable with respect to the local situation.

There is a chance that proper seeding techniques applied at the optimum time can add an appreciable amount of water to the reservoirs. As in any pioneering venture, chances must be taken. If taking such chances does not involve prohibitive costs and if the experiments are planned in such a manner that a scientific evaluation is possible after a reasonable time period has elapsed, then the money and effort are not wasted. If seeding operations over the New York watersheds can be shown to have no apparent economic value, even this information is valuable to have.

We do not yet know which factor or combination of factors are of primary importance in the development of precipitation. Much progress has been made in this field during the last few years and new developments are taking place rapidly. As experience is gained in the Northeast, the Rockies, the semi-arid regions of the Southwest, in California, Australia, Switzerland, France, Canada, Cuba, South Africa, and the many other parts of the world where techniques in experimental meteorology are being developed, the possibilities and limitations of this new science will gradually become delineated.

Those responsible for the establishment of a working experiment in the New York region are to be congratulated on their persistence in carrying out their plans despite various types of obstacles. The over-all value of this activity, quite regardless of its outcome, will be far and above the value of the water that might be added to the pipes flowing into New York City. It is my personal feeling that the type of experiment under study by New York City is one of the most difficult in experimental meteorology. By contrast, the prevention of local hail storms, lightning, and torrential rains should be easy. Such things are relative, however, and although we can now suggest some of the possibilities and limitations in this field of endeavor, there remain many problems in transposing ideas and theories to practical applications.

As in the development of any idea based on an assemblage of scientific facts, progress is sometimes slow and discouraging; at other times, it is rapid and breath-taking. Experimental meteorology seems destined to enjoy both types of progress. Wild speculation is particularly easy with a subject so close to all of us as the weather.

With the establishment of a stable scientific structure as its basis, the true facts will emerge at a speed consistent with the industry, imagination, enthusiasm, and integrity of the individuals and organizations concerned with its development. I see no reason at the present time why it should not become one of the important developments in meteorology in the years ahead.

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THE IMPORTANCE OF ARTIFICIAL NUCLEATION FOR THE PRODUCTION OF PRECIPITATION*

By WILLIAM LEWIS†

One of the most important questions involved in current discussions of the effects of cloud seeding is the problem of the reliability of natural ice-forming processes in the atmosphere. Since the proponents of artificial rainmaking are willing to concede that the meteorological conditions must be "suitable," we must consider carefully just what conditions are indeed suitable for the formation of appreciable precipitation and whether natural processes can be relied upon for the release of moisture under these conditions. The basic question is this: how frequently does it happen that the occurrence of precipitation is prevented by an absence of ice-forming nuclei when conditions are otherwise favorable for the occurrence of rain?

My own opinion on this question is that the basic cause of precipitation is the lifting of moist air, and that when suitable conditions of lifting occur, the necessary ice crystals are formed by natural processes. Any exceptional cases in which the occurrence of precipitation may be prevented by a lack of ice-forming nuclei are so rare or the amounts of moisture involved are so small that their importance is trivial.

First let us consider the type and composition of clouds in those situations in which sufficient lifting occurs for the formation of appreciable rain. The formation of light to moderate precipitation from stratiform clouds requires sufficient horizontal-velocity convergence in the lower layers over a considerable area to produce average vertical velocities of the order of 1,000 feet per hour, affecting a layer of the order of 10,000 feet or more in vertical extent. This process causes the formation of very deep, continuous clouds of the type known as "nimbostratus" or "altostratus." If a lack of nuclei were an important factor in preventing rain in cases of this kind, one would expect to find supercooled liquid clouds of from 5,000 to 10,000 feet or more in vertical extent. Observations show, however, that in all cases investigated cloud systems of this type are composed mainly of ice crystals in the portions above the freezing level. Investigations of the extent and liquid-water content of supercooled clouds of various types have shown that supercooled cloud layers seldom, if ever, exceed 3000 feet in vertical extent.

The other principal type of rain-producing cloud is the "convective" or "cumulonimbus" type. Clouds of this type consist of local updrafts, usually rising to considerable heights and characterized by relatively high vertical velocities. Three types of convective clouds are generally recognized: (1) cumulus humilis, clouds having relatively slight vertical development; (2) cumulus congestus, clouds with greater vertical development but without spreading or softening of the tops; and (3) cumulonimbus, clouds with great vertical development, characterized by spreading or softening of the tops and precipitation or virga from the bases. It is generally agreed that a cumulus congestus cloud becomes cumulonimbus when ice crystals are formed within it.

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* On May 23, 1950, the Section of Oceanography and Meteorology held a symposium, presided over by A. F. Merewether of Pan-American Airlines, on "Induced Precipitation." This paper was the third of four presented at the symposium.

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The amount of rain that may fall from a cumulonimbus cloud depends on the amount and moisture content of the air included in the updraft, the amount of mixing with the environment, and the vertical extent of the updraft. The first factor mentioned, the amount of air carried upward in the rising column, is of greatest importance. In cases where the warm moist layer near the surface is unusually thick or when its thickness tends to increase due to horizontal convergence, the ample supply of moist air produces updrafts which are likely to continue for a considerable time, finally reaching a stable layer and spreading out at the top. If, on the other hand, the warm moist layer is shallow, cloud development is often arrested due to local exhaustion of the moisture supply, and the clouds subside and dissipate by mixing with the surrounding air. Since the amount of rain that may be produced depends upon the amount of air that is lifted, most of the precipitation from cumulus-type clouds should be expected from those in which the updraft continues for a considerable time.

Careful observations of the development of cumulus and cumulonimbus clouds and a study of lapse-time motion pictures indicate that the lifetime of a single cumulus congestus tower is usually only about 10 to 20 minutes. These clouds nearly always either subside and dissipate or become cumulonimbus by the formation of ice crystals within that time. It is significant that, when a cumulus cloud does not subside but continues to rise after it has been visible for about ten minutes, it almost invariably starts to bulge at the top and shows evidence of ice crystals within the next four or five minutes. These remarks are based on my own observations of cumulus cloud development in California, New Mexico, New York, and Virginia, and on a study of photographs and lapse-time motion pictures made by "Project Cirrus."

These observations have led me to propose the hypothesis that the natural process of ice formation in clouds usually requires that the cloud-mass involved must exist at a fairly low temperature for a certain time interval, which is probably a function of the temperature (for example, about ten minutes at −15°C and longer periods at higher temperatures). On the basis of this hypothesis, the principal factor which determines whether a particular cumulus congestus cloud will become cumulonimbus is the duration of the updraft. If the supply of warm moist air gives out after about 10 minutes or less, the cloud will dissipate rapidly by mixing with the environment without the formation of ice crystals, while, if the updraft is sustained for 20 minutes or longer and reaches temperatures below about −10°C, the transformation to cumulonimbus is almost certain to occur.

It should be noted that it is just these clouds, consisting of sustained updrafts involving large amounts of moist air, that can be expected to produce appreciable amounts of precipitation. Updrafts which last for a few minutes only do not ordinarily contain sufficient moisture to produce more than traces of rain or very light showers.

Let us consider what would happen if, due to an absence of ice-forming nuclei, a sustained and vigorous updraft should fail to change to ice crystals. Since the cloud could not continue indefinitely upward, it would spread out at the top, forming a sort of anvil, which, however, would be altocumulus in form rather than cirroform. Altocumulus clouds of this type, formed by the spreading of cumulus cloud tops, are sometimes observed but usually only when the stable layer which limits the updraft occurs at a temperature above freezing or at least above −10°C. I have never personally seen a spreading top on a tall cumulus cloud which was not fibrous in structure, showing the presence of ice crystals.

This hypothesis concerning the time factor involved in the formation of ice crystals is also consistent with observed facts concerning the composition of layer-type clouds of various kinds. The most common type of supercooled layer cloud is the stratocumulus or altocumulus which is formed in the upper portion of a turbulent layer of air. The thickness of this layer of turbulent mixing is usually substantially greater than the thickness of the cloud, since condensation only occurs in the upper, cooler portions of the layer. The individual parcels of fog which make up the cloud are therefore being continuously formed by condensation in the rising currents and dissipating by evaporation in the descending currents. Thus, the life of an individual parcel of condensed fog is only a few minutes, although the cloud layer as a whole may last for several hours. The reality of this process of continuous formation and dissipation is clearly shown in lapse-time motion pictures of broken stratocumulus clouds. According to my hypothesis, the reason ice crystals are not usually found in clouds of this kind is that the life of the individual fog parcels is less than the time required for the formation of ice crystals

This turbulent mixing process gives rise to layers of limited thickness, usually not exceeding 3000 feet. Very thick cloud layers, on the other hand, are generally produced by the lifting of large masses of air by convergence, as was mentioned earlier. In clouds of this type, the individual parcels remain in a saturated state for long periods, allowing ample time for natural ice-crystal formation.

To sum up the argument thus far, it has been shown that the types of cloud which can produce appreciable amounts of precipitation (that is, those formed by the gradual lifting of extensive air masses and those formed by sustained local updrafts involving large amounts of air) are just the types in which the natural formation of ice crystals is observed to occur with great regularity. These are facts of observation. In addition, a hypothesis has been proposed which is consistent with these observations and also with the fact that ordinary stratocumulus and altocumulus layers and short-lived cumulus congestus clouds are generally composed of supercooled drops.

Since some types of clouds are frequently observed to be supercooled, let us consider how much precipitation could be obtained by converting them to ice. Calculations based on the values of liquid-water concentration measured by the National Advisory Committee for Aeronautics show that a typical supercooled stratocumulus layer would yield almost enough precipitation to reach the ground, while one cloud in 100 would produce as much as .01 inch. Similar calculations for cumulus clouds show that as much as .01 or .02 inch could be expected frequently and, in extreme cases, as much as .10 inch might possibly be obtained. It is extremely doubtful that a cloud capable of producing more than .10 inch would fail to precipitate naturally.

So far I have not discussed the mechanism of ice crystal formation in clouds: what kind of nuclei are required, or whether these act as sublimation nuclei, as freezing nuclei, or as "motes" which promote the freezing of cloud drops. A considerable amount of information concerning the formation of ice crystals in the laboratory under certain conditions is now available as a result of work done by several investigators. I will not discuss these experiments in detail, but would like to point out that the conditions occurring in the clouds have not been reproduced in the laboratory. It is not possible, therefore, on the basis of laboratory experiments, to conclude that natural air does not contain whatever particles may be necessary to promote the formation of ice crystals in a cloud that has aged for, let us say, 15 minutes at −10°C. Observations of ice crystal formation in a cold box on Mt. Washington indicate that the concentration of ice crystals formed under the conditions of the experiment is sometimes very small, but this does not prove that natural ice crystals would not form in the same air under the conditions prevailing in natural clouds. In my opinion, observations of the composition of natural clouds furnish much more convincing evidence on this point. The fact that many cases of extensive cloud systems more than four or five thousand feet thick and with temperatures below freezing were observed to be composed mainly of ice crystals, without exception, is very significant evidence of the dependability of natural ice-forming processes.

Let us consider for a moment the results of actual field tests of cloud seeding. In the case of layer-type clouds, the fact of the possibility of cloud modification has been very convincingly demonstrated. Equally convincing is the failure of all seeding experiments on layer clouds to produce more than trivial amounts of precipitation.

In the case of cumulus clouds, the evaluation of the results of seeding experiments presents a very difficult problem, due to the impossibility of determining, in any particular case, what the natural course of the weather would have been if the experiment had not been performed. The only way that the effects of seeding cumulus clouds can be honestly evaluated is to make as many experiments as possible and study the effects in all cases, observing at the same time the behavior of unseeded clouds nearby. The Weather Bureau, in cooperation with the Air Forces, has conducted two different series of tests on cumulus clouds. The reports of these tests include tables showing, for each seeding operation, the condition of the cloud at the time of seeding, its subsequent development, and the amount of precipitation. The general result of these tests is to show that the most frequent occurrence following seeding is partial dissipation of the seeded cloud and that, occasionally, small amounts of precipitation occurred, but only when natural precipitation was occurring nearby.

Experiments conducted in Canada gave somewhat more favorable results. Of a total of 33 trials with supercooled cumulus clouds, 36 per cent gave no precipitation, 27 per cent gave only virga, and 36 per cent were followed by precipitation reaching the ground. In 8 out of 12 of the cases of rain reaching the ground, natural rain was also occurring within 25 miles. The other four cases were all of the "selective" type, that is, an effort was made to choose the most favorable cloud in the area. Under these conditions, there is a good reason to suspect that these showers would have occurred without seeding.

In summary, my point of view is that, although cloud modification by nucleation has been demonstrated, significant changes in the amount and distribution of precipitation can not be accomplished by cloud seeding, since the distribution of precipitation is determined primarily by air circulation rather than by nuclei. Whenever circulation patterns exist which involve sufficient lifting for the production of appreciable amounts of precipitation, the natural processes of ice-crystal formation are effective in releasing the available moisture.

The possibility remains, however, that the character of precipitation might in some cases be modified by seeding, and I believe that, if successful practical applications of cloud seeding are to be found, they are likely to lie in the field of hail-prevention rather than rain-production. Any estimate at this time of the probability of success would be only a guess, but it appears to me that experiments along this line would be worthwhile.

LARGE SCALE CONTROL OF WEATHER BY INTRODUCTION OF SUBLIMATION NUCLEI INTO THE ATMOSPHERE[*]

By WILLARD J. PIERSON, JR.[†]

During the past few years, a great many statements have been made concerning the possibility of producing rainfall and controlling the weather by the introduction of sublimation nuclei into the atmosphere. In fact, some claims of success have appeared in the press. It is the purpose of this paper to point out that some of the statements which have been made are not founded in fact, that the techniques which have been employed are inadequate, and that, in most of the studies which have been conducted, there has been an overall disregard of the scientific facts upon which the theory of meteorology is based.

In the preceding paper, Mr. Lewis has given some reasons for his belief that the theory upon which the possible control of the weather is based is not well founded. His contention that there is always a sufficient number of nuclei present in the atmosphere to produce all of the rainfall that would naturally occur seems to be well supported. There is a danger that too many nuclei will be released into the atmosphere. Too many nuclei prohibit the production of rainfall by causing the formation of a great many small ice crystals which are too small to fall and grow into large rain drops when melted. Dr. Schaefer has done a fine job in measuring the sublimation nuclei count at Mount Washington. However, many more observations, tabulated according to air mass, elevation, air trajectory, and cloud type, are needed before it will be possible to prove that situations exist in which artificial nucleation is advisable.

One of the claims of success which has been made will now be discussed. In a recent article in "Life," dated February 20, 1950, the claim was made that a cloud about twenty-five miles away from a silver iodide nuclei generator was seeded by the silver iodide and developed into a full scale thunderstorm. In addition, it was also claimed that the silver iodide spread throughout a large part of the state of New Mexico and "brought down 394 billion gallons of water" and "that dry creek beds changed into raging rivers."

Consider first the cloud about twenty-five miles away. The following facts can be found in the magazine article and in another paper published on the experiment.[1] Seeding was started at about 5:30 A.M. The cloud base which developed was at about 12,000 feet M.S.L. when it formed at about 8:30 A.M. The cloud grew at the top at the rate of 160 feet per minute and reached 26,000 feet and a temperature of −23° centigrade at 9:57 A.M. Then the cloud shot up at a rate of 1200 feet per minute until it reached 44,000 feet and a temperature of −65° centigrade at 10:12. Rain started from the cloud at 10:06.

Now, the silver iodide was released at the *ground* twenty-five miles away. To be effective, it must have risen in the cloud until it reached a temperature of −5° centigrade at a height of 19,000 feet M.S.L. Albuquerque is at an elevation of approximately 5,000 feet M.S.L., so the silver iodide must have ascended 7,000 feet to reach the base of the cloud and about 7,000 feet more to reach the temperature at which it becomes effective.

The environment of a building cumulus cloud is generally sinking, so the silver iodide must have traveled along the surface. The surface winds were ten miles per hour toward the cloud and, if the silver iodide actually reached the area below the cloud (which it may not have done), it would require two and one-half hours to travel the distance. The silver iodide might then have been under the cloud at 8:00 A.M. To ascend 7,000 feet to the base of the cloud in two hours would require an average vertical velocity of 58 feet per minute or 29 centimeters per second.

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[*] On May 23, 1950, the Section of Oceanography and Meteorology held a symposium, presided over by A. F. Merewether of Pan-American Airlines, on "Induced Precipitation." This paper was the fourth of four presented at the symposium.

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The vertical velocity beneath a cumulus cloud in the building state is very small at the surface and increases with elevation. Byers and Braham[2] have measured these velocities in typical building cumuli, and they found a vertical velocity of about 120 feet per minute at 7,000 feet. From the surface to 7,000 feet, however, the vertical velocity is certainly less than this value. If the vertical velocity were to increase linearly from the surface to 7,000 feet and if it had a value of 120 feet per minute at 7,000 feet, the silver iodide would just barely reach the base of the cloud at 10:00 A.M. The silver iodide must still ascend 7,000 feet more before reaching a temperature of −5° centigrade. Byers and Braham[2] have shown that the vertical velocities increase with height within a building cumulus. The top of the cloud was rising at the rate of 160 feet per minute during this stage of the development, and, if it is assumed that this value of the vertical velocity existed from the base of the cloud to the level at which the temperature was −5°C, forty more minutes would still be required for the silver iodide to reach a level where it could become effective.

Turbulent upward diffusion in the lower levels has been neglected and may modify the results slightly. Also, the silver iodide was permitted to rise for one-half hour before the cloud actually formed. These calculations indicate, however, that the cloud should not have been triggered into a thunderstorm until 10:40 by the silver iodide. Therefore, a doubt arises as to the claim that the silver iodide actually triggered the storm.

The cloud had reached the level of 26,000 feet and a temperature of −23° centigrade when it suddenly spurted upward and developed into a thunderstorm. At such temperatures, almost any natural sublimation nuclei could have triggered the cloud. Before it can be proved that silver iodide released from the ground actually caused a thunderstorm, the path followed by the silver iodide must be accurately traced and it must be proved that the silver iodide had reached a point in the atmosphere where the properties of the silver iodide became effective.

As for the 394 billion gallons and the dry creek beds changed into raging rivers, a cold front which could be located to the north of Albuquerque two or three days before the seeding experiment took place moved steadily through New Mexico and passed Albuquerque at the time the thunderstorm started. All of the rainfall can be explained by the activity of the cold front. Finally, a typical occurrence in New Mexico is a so-called "flash flood," and the dry creek beds are there because the flash floods have produced them.

The rainmaking attempts at the New York watershed will now be considered. Dr. Howell should be highly complimented for his conservative evaluation of the results of his experiments. A rigorous statistical study must be made after the experiments have been completed in order to discover if the hoped for 5 per cent increase over what would have been the natural rainfall without seeding is real or illusory.

For one seeding experiment which has been conducted, however, it will be shown that the silver iodide could not possibly have produced any rainfall. The experiment took place on April 25 and April 26th. Silver iodide was released at the surface at Stroudsburg. Representative atmospheric soundings at Albany show that there was a 6°C temperature inversion over New York State during the time that the silver iodide was being released. The air into which the silver iodide was released was therefore capped by an inversion, and the lapse rates were such that it would be impossible for the silver iodide to penetrate to the freezing level where it becomes effective. In fact, the silver iodide could not possibly have risen above four thousand feet, which was the height of the base of the inversion.

Meteorology is a science which demands careful consideration of many complex factors. Suppose, for example, that one wanted to increase the rainfall at New York City from a warm front situation. It would be futile to release silver iodide in New York State during warm front rainfall, because the silver iodide could not rise through the cold stable underlying air to the freezing level above in the more moist and potentially warmer air.

Consider a warm front in which the frontal surface slopes upward toward the north and rises one mile over a horizontal distance of 100 miles. For simplicity, let us assume that the winds in the warm air are tangent to the sloping frontal surface and that they have a velocity of 40 miles per hour. Let the freezing level be at 10,000 feet, or approximately 2 miles. If the silver iodide is released at the surface, it must be released at a point 200 miles south of the desired location of the rainfall. It would reach the area where the rainfall was desired about 5 hours after its release at the surface.

Actual weather situations are more complex than the simple model just described. The silver iodide would probably have to be released at the surface in Arkansas or Missouri before it could affect New York State during a warm front regime. Dr. Helmut Weickmann, in a recent seminar at New York University, showed by some theoretical calculations that doubling the number of available nuclei during a warm front rain might possibly increase the rainfall by 5 per cent during light rains, but too many nuclei might actually cause less rain.

In view of the points which have just been considered, it can be concluded that the large scale control of the weather is still only a distant future possibility, that the global nature of the weather has been completely neglected in most of the work which has been done, and that in no case has it been definitely proved that economically significant amounts of rainfall have been produced.

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DIVISION OF MYCOLOGY

THE OCCURRENCE AND INDUCTION OF MUTATIONS IN THE FUNGI*

By EDWARD J. BACKUS†

The occurrence of mutations in microorganisms has been recognized for a great many years. Those who have handled filamentous fungi are familiar with the sectoring phenomenon so frequently displayed when single cell colonies are grown on an agar plate. Although there was once reluctance on the part of some investigators to consider these variants to be true mutants, it is now well established that sectors arise when mutations occur in one of the cells of the developing colony. As such a mutated cell proliferates into a mass of "abnormal" cells, the typical wedge-shaped sector becomes apparent. While gross morphological mutation has been demonstrated in practically all classes of the fungi, the frequency with which it occurs varies substantially among the different genera and species comprising these groups. Equally common in occurrence are the mutant forms which, while resembling the parent type both structurally and culturally, show major differences in biochemical and physiological behavior.

The problems created by this natural tendency for fungi to mutate fall heavily upon those concerned with taxonomic considerations, especially in the large, diverse groups such as the Fusaria, Penicillia, Aspergilli, or the Actinomycetes. The separation of species, or even genera, in the Actinomycetes, is often based upon differences so small that they may be bridged by the deviations occasioned by mutation. The widespread and varied positions which fungi occupy in man's present-day scheme of living have caused the incidence of mutant strains to become important economic factors. For example, the plant breeder and the agriculturist must fear the sudden appearance of "races" of plant pathogens with altered pathogenicity potentials which may be capable of attacking supposedly resistant varieties of economic crop plants. Likewise in the fermentation industries, where so much is based upon an organism's capacities for producing small quantities of certain metabolic products, constant guard must be kept against mutants of lesser capacity dominating the master culture.

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* This paper, illustrated with lantern slides, was presented at the meeting of the Section, May 26, 1950

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Observations that the rate of appearance of mutants in the fungi, particularly in culture on artificial media in the laboratory, was influenced by such environmental factors as temperature, light, pH, accumulation of staling products, salt concentrations, and medium constituents led to organized plans on the part of many investigators to apply variations of these and other conditions in an attempt to cause greater numbers of mutations to occur. The main phase of this discussion is concerned with the progress that has been made in the study of this problem of the induction of mutations in the fungi. Historically, this work goes back about forty years, but it is only within the last decade that microorganisms have become favorite subjects for the study of mutation and inheritance. That they should ultimately have attained this position is not surprising when the tremendous populations that may be worked with, the rapid turnover of generations, and the facility provided for rigidly controlling conditions for all members of a population are compared with the much slower generation time and greater difficulty involved in controlling conditions uniformly in the higher plants and in animals.

The agencies found to be useful in the stimulation of organisms to increased rates of mutation fall chiefly into two categories, radiations and chemical substances. Before proceeding to a consideration of the radiation work, it may be well to point out that the original observations on the mutagenic potentialities of many of the agencies have been made in connection with studies on *Drosophila* or other higher organisms and only later applied to microbiological investigations. Thus, the discovery by Muller in 1927, that mutants can be produced artificially in *Drosophila* by X-ray treatments, and his observation that the induced mutation rate was far greater than the natural rate of mutation in the organism provided the impetus for the tremendous volume of related X-ray work which has followed. It is impossible to point to any single investigation which supplied a similar stimulus for the use of ultraviolet light as a means of inducing mutations. Although the lethal action of ultraviolet on bacterial cells was clearly established as early as 1901 by Bie, the fact that some of the organisms surviving such treatments are mutated was not recognized for many years. The failure of this agency to receive the attention it deserved may be due in part to its comparative ineffectiveness in much of the early induced mutation work on higher plants, a situation undoubtedly resulting from the absorption of the ultraviolet rays by somatic tissues intervening between the light source and the germ cells.

After working out a method with Claus in 1936 whereby each bacterial cell, yeast cell, or fungus spore could be exposed to approximately equivalent amounts of monochromatic radiation, Hollaender collaborated with Emmons in 1939 in a study of the induction of mutations by ultraviolet radiation in the dermatophyte *Trichophyton mentagrophytes*. This was the first major attempt to correlate the effects of wave length and energy absorbed per spore with the rate of mutation. This and subsequent investigations have demonstrated that an ultraviolet wave length of 2650 Å is both the most effective in killing fungus spores and the most efficient in the induction of mutations. Wave lengths of 2280 Å produce high killing rates and low mutation rates, a situation possibly related to a protective action in the fungi associated with the high continuous absorption of rays in the 2200 Å region by the chitin in the spore walls. Wave lengths of 2967 Å give low rates of both mutation and killing, while those longer than 3400 Å give rise to no genetical variants. It has been suggested that the absorption of ultraviolet light by the nucleoproteins of the chromosomes possibly leads to the genetic effects recorded, the nucleic acids, which show a high absorption coefficient at the 2650 Å wave length, being the active agents in the process. Hollaender and Emmons observed that the rate of mutation increased with energy absorbed per spore (that is, dosage or time of exposure at a constant wave length) up to a certain maximum level and then decreased with further increase in energy. Although this response has not been observed in a few of the ultraviolet studies of mutation in the fungi, the majority of subsequent investigations have been substantiating in nature. Hollaender and his collaborators have indicated that even higher rates of mutation can be attained by the incubation of irradiated spores in physiological salt solution for a few days before plating out on agar. Ultraviolet radiation may cause many organisms to display aberrant forms, sometimes designated as "degenerate phenotypes," which are characterized by a tendency either to die out or quickly to revert to normal after a few generations. It is thought that this type probably is not the expression of any genetic change, but rather is due to a cytoplasmic effect.

Application to the fungi of the principle of inducing mutations by X rays did not assume major importance until it was used on *Neurospora crassa* by Lindegren and Lindegren in 1941, and even then only small attention was given to correlation of dosage to rates of mutation. More concrete data on the method was provided by later investigations, such as that of Sansome, Demerec, and Hollaender on *Neurospora* in 1945, which indicates that the rate of mutation with X-ray exposure increases with dosage in a linear manner and is higher at any given dosage when the radiation is applied in high intensity (5,400 r./min.) than when the same dosage is given in low intensity (240 r./min.). It has been established that X rays cause a great many actual chromosomal aberrations and rearrangements. Changes in an organism which may be attributed to such causes cannot, if one adheres to the strictest terms of the definition, be considered true mutations. Most investigators include these variants among the mutant forms, however, since it is frequently not feasible to detect the actual causes for the modifications observed.

A wide variety of other radiations have been used to stimulate mutation rates in the fungi, including among others cathode rays, gamma rays, neutron bombardment, and uranyl nitrate. In general, results achieved with these agencies have been positive but with rates of induced mutation falling far below those attainable with X ray or ultraviolet ray. While near infrared radiation alone exerts no recognizable effect on mutation rates or chromosome rearrangements, Hollaender and Swanson have demonstrated with *Aspergillus terreus* that a pretreatment with near infrared at 6,000–20,000 Å will significantly increase the rate of X ray-induced mutations as compared to that of X-ray controls.

Practical application of the capacity of radiations to induce mutations in microorganisms has been made to further the advancement of other fields of biological investigation. The fact that mutant strains of an organism may show varied biochemical activities has led to an extensive study of such forms in relation to metabolism, particularly in *Neurospora* and *Penicillium*, with a resultant elucidation of a number of steps in the synthesis of various vitamins and amino acids. Most active investigators in this field have been Beadle, Tatum, Bonner, Horowitz, Mitchell, and their many associates. Beadle postulates that if proper conditions of experimentation and testing can be set up, the probability is high of obtaining mutations concerned with almost any synthetic process in an organism. Deficiency mutants with altered nutritional requirements have proven to be highly useful as assay organisms for such substances as *p*-aminobenzoic acid, choline, and leucine, growth responses being comparatively proportional to the amount of the factor present in the material being tested. The induction of mutations by radiation treatments has played a major role in the development of antibiotics produced by microorganisms. Outstanding in this respect is the genealogy of *Penicillium chrysogenum* in relation to penicillin production, with the major successes being recorded in the development of X-1612 at the Carnegie Institution of Washington, Biological Laboratory, Cold Spring Harbor, N. Y. by X-ray treatment of conidia of NRRL 1951-B25, and the subsequent isolation of mutant strain Q176 at the University of Wisconsin following ultraviolet treatment of conidia of X-1612. No doubt major improvements have been made in the organisms producing streptomycin and chloromycetin by induced mutation studies in the industrial laboratories. I can report from personal experience that ultraviolet radiation has been used to good advantage in our laboratories in the induction of mutants of *Streptomyces aureofaciens* capable of producing quantities of aureomycin well in excess of the capacities of the original soil isolate.

Historically, the search for chemical mutagens is very old. Even after the demonstration of the effects of radiations on mutation rates, the quest for chemical agencies was not abandoned, since it is generally believed that mutation is a chemical process. Therefore, the possibility exists that a known chemical substance might provide a better lead as to the nature of the gene itself. The geneticists' regard of the radiations as too drastic and indiscriminate in their action supplied added stimulus to this search. Probably the greatest hazard attendant upon the use of chemicals to induce mutation is that of achieving penetration deep enough to reach the chromatic materials of the cells without having to use concentrations that are so strong that excessive killing results.

While the first well-organized approach to the subject in the fungi was made in 1939–40 by Steinberg and Thom when they attempted to induce mutations in *Aspergillus niger* by altering gene proteins with a variety of chemical agents, the most effective of which proved to be $NaNO_2$, the subsequent demonstration in *Drosophila* by Auerbach and Robson that mustard gas is a powerful mutagenic agent provided the real impetus for the development of this field. Soon included in the scope of their work was a series of chemically related compounds, sulfur mustard and the nitrogen mustards. All of these substances are strong vesicants and all proved to be excellent agents for the induction of mutations in *Drosophila*.

Confirmatory evidence of the efficiency of mustards as mutagens for microorganisms has accumulated rapidly in the last four years. Mutation rates in *Neurospora crassa* treated with nitrogen mustard were found to be similar to those obtained by ultraviolet radiation, and even higher rates were reported when germinating spores were treated. The data of Tatum and of Giles suggests that the types of mutants induced in *Neurospora* by the various mustards are both quite similar to each other and to the ultraviolet derived mutants, those characterized by an inability to synthesize methionine being the most frequently occurring class of biochemical variants appearing as a result of treatments with any of these agencies. Other microorganisms which have responded to nitrogen mustard treatments with increased mutation rates include *P. notatum*, *Coprinus fimetarius* *Aspergillus nidulans*, and *E. coli*.

The understanding of the mode of action of the mustards has not advanced far beyond the hypothesis stage. A similarity between the genetic effects of the mustards and of X rays has been suggested by the *Drosophila* studies, the main points of difference being in the greater number of chromosomal rearrangements occurring in the irradiated material and the apparently delayed mutation which may result following mustard treatment. Since it has been chemically demonstrated that the mustards react with various amino acids, such as arginine, methionine, and threonine, it is reasonable to suppose that these highly reactive substances, with their unsaturated nitrogen or sulfur atoms, may possibly combine with the proteins of the genetic materials of the cell and alter them in such a manner as to cause a change in the subsequent manifestations of the organism.

So long and diverse is the list of chemicals which have been tried as mutagenic agents in recent years, that we must limit our attentions here to a brief mention of those which have produced positive reactions in tests on microorganisms. The alkaloid, colchicine, so widely used in higher plants to produce polyploid forms, has been used to induce "giant" races in *P. notatum* but generally has enjoyed very limited success and usage in mycological work. Included in the list of substances shown to induce at least slight increases in mutation rates in microorganisms are camphor, allyl isothiocyanate, various carbamates, benzimidazole, acriflavine, theophylline, and caffeine. Very recently, Demerec has shown that a variety of carcinogens belonging either to the polycyclic hydrocarbons or to the azo- compounds are capable of inducing mutation in *Drosophila*. Little is yet known concerning the reaction of fungi to these agencies. The scattered favorable reports of the use of miscellaneous chemical compounds to induce mutations in the fungi serve as the basis for continued interest in further exploration of the field.

Recent years have seen a growing interest in combination treatments to induce mutation, although their use has been almost exclusively limited to animal and higher plant studies. However, Swanson and Goodgal have demonstrated that weak, nonmutagenic doses of nitrogen mustard followed by a normal ultraviolet radiation will markedly increase mutation rates in *A. terreus* over and above that obtainable with the radiation treatment alone.

Stone and his associates have conducted a series of experiments with *Staphylococcus aureus* which have shown that pretreatment of media with ultraviolet light induces marked increases in the rate of mutation in organisms subsequently grown upon it. Similar results were recorded when 1 to 6 ppm. of hydrogen peroxide were added to the medium in place of the irradiation treatment. More recently, the method has been applied to *Neurospora crassa* and extended to include a series of organic peroxides, all of which show mutagenic potentialities. It has long been known that ultraviolet radiation of water produces considerable quantities of hydrogen peroxide. This recent work adds evidence in support of the hypothesis that ultraviolet light possibly increases mutation rates because of the formation of peroxide compounds in media or cells subjected to radiation.

It is hoped that his brief review has emphasized the great progress that has been made in the induction of mutations during the last fifteen years. This study is now a vitally active, growing field of scientific endeavor. Although an absolute comprehension of means by which the various agencies bring about changes in the genetic materials of the organisms is not possible at this time, working theories and hypotheses have been advanced, and real progress toward this understanding is being made.

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OPENING AND DEDICATION OF THE ACADEMY BUILDING

On April 11, 1950, The New York Academy of Sciences officially opened its new home at 2 East Sixty-third Street with a formal reception marking the beginning of the celebration for The Academy's Members and their Guests. The dedicatory program was as follows: President Crossley gave the address of welcome; Doctor Detlev W. Bronk, President of John Hopkins University, delivered an address emphasizing the role of The New York Academy of Sciences as a forum for the presentation and discussion of scientific ideas. Doctor George B. Pegram, Vice-President of Columbia University and of the Academy, dedicated the new building.

The Academy held open house with reception, from three to seven o'clock, on each of the four days from April 11th to April 14th, for the Members and their Guests. Attendance for the period totaled 2,592. A special feature of the House Opening Celebration was an exhibition of paintings and sculpture by Joel Stolper, well-known animal painter; an exhibit of photographs of famous scientists by Doctors Harden F. Taylor, J. A. Aeschlimann, and M. F. Furter; as well as two flower paintings by Sidney Hatchell, of Mystic, Connecticut. The gardens and floral decorations were arranged by Mr. Hatchell. Music was rendered by an orchestra under the direction of Jerry Lloyd.

The Address by President M. L. Crossley follows.

ADDRESS OF WELCOME

By PRESIDENT M. L. CROSSLEY

Great occasions, like great deeds, result from persistent endeavor. This is such an occasion; the culmination of a long struggle to secure a home for the Academy. Faith, hope, and good works, welded into an effective force by the gracious, persuasive, dynamic personality and indomitable will of the Executive Director, Mrs. Eunice Thomas Miner, have finally achieved success and brought us to this historic moment of jubilation, which we wish to have you share with us.

We welcome you to our party; we are glad you could come. You will see what we have now and think of its potential possibilities. Some of you will be privileged to participate in the scientific work of the Academy and will enjoy the service. Others will have the satisfaction of knowing that their contributions to the physical resources of the institution will have helped, in time, to establish the greatest scientific center in the world.

We are grateful to all who have helped us to realize our aims thus far. We hope to have their continued interest and help—both members and friends. Particularly do we, at this time, acknowledge our great debt of gratitude to the Woolworths for their generosity and splendid foresight in making this building available to the Academy for a scientific center in New York. Also, we are not unmindful of the debt we owe to all who have served the Academy so well throughout the long years of its past history and who have made this occasion possible. Their contributions are fused into the very life of the institution and help to guide its future course of action. To the Officers of the Council, Committees, and Administrative Staff; to the architect, Mr. W. Stuart Thompson, and the other friends whose counsel and aid have been so generously given, we express our deep appreciation and offer sincere thanks.

One hundred and thirty-three years is a long time in the life of this nation. On February twenty-fourth last, the Academy attained this ripe old age, but without any signs of senility. On the contrary, it is a vigorous, active, purposeful institution with great capacity for service to the sciences. Its plan of action enables investigators of the different phases of a problem to report their results and to confer with one another regarding the significance of their findings. These reports, when thus evaluated and published in the Annals of the Academy, serve as full and authoritative records of what has been accomplished. The conferences, under expert leadership, endeavor to cover each subject as fully as possible, giving the scientific investigators in the field the opportunity to present and evaluate critically all the information available, so that the knowledge resulting will be a full and complete unit for use of other interested investigators. This purpose is steadfastly maintained. Solidarity in action is dependent upon oneness of purpose. This the Academy has.

Individual fragments of knowledge, like gold dust in alluvial soil, have no value until assembled, correlated, and evaluated. The conference plan permits the Academy to seek those who have fragments of experimental evidence and gems of thought which they can fit into a composite picture to reveal, not only the true significance of what has been accomplished, but, what is usually equally important, the missing fragments needed to give the picture reality and value. There are many facets to a scientific gem of knowledge. The most important of these are seldom obtained by direct application of wholly new concepts. What appears to be new is generally a composite of the old and the new, with never before recognized implications and challenging significance so striking as to obscure its true symbiotic nature. Such knowledge more often lies at the apex of a pyramid whose base and sides are composed of an intricate mosaic of supporting facts derived from the information secured as a result of many different types of investigations, touching on various phases of the problem. The interfusion of related facts leads to the knowledge required to chart the course to follow for new discoveries.

The role of the Academy in helping to see that the results of scientific investigations become available in the world of affairs can go on now with accelerated speed. Better facilities will enhance the service rendered by the many scientists who labor willingly and loyally for this institution and the public it serves. Buildings and equipment are only auxiliary to this service, however, and, in the main, the work of the Academy, as in the past, will be measured in terms of the caliber of the people who serve it. The greater the achievements of science and technology, the more need there is for vision and competence in those who recognize and interpret scientific results.

Any appraisal of what lies ahead in opportunities for the Academy must be based on an understanding and appreciation of its work in the past and the relation of this to the advancements in the sciences. Much of great significance has happened in science in the United States and in the entire world during the life span of the Academy. The combined powers of science and technology, aided by industry, have brought into being materials and things which have greatly improved human well-being. Basic to this accomplishment have been the discoveries of the essential features atomic and molecular architecture responsible for the properties of matter; the nature of disease and its causative agents; the essential biochemistry involved in many of life's processes, including glimpses into the marvelously interesting and complicated steps controlled by specific chemical substances such as enzymes and hormones produced by definite biological structures when needed; genes and their role in heredity; the chemotherapy of infectious diseases, with synthetic drugs and antibiotics saving many from untimely death; the causative agents in certain cancers and the transplantation of cancers in animals, with accumulating knowledge of their common properties and requirements, as well as the conditions required for their growth; the growth of cancer tissue outside of the body in artificial media and the transformation of normal cells into cancer cells which can then be implanted into animals, survive, and grow resembling types of spontaneous cancer; the mutation of plant and animal cells; and the realization of the alchemist's dream of the transmutation of the elements. These and many more equally important discoveries have resulted from the work of scientists in many different laboratories in many countries and have added to the common store of knowledge available to all mankind.

The Academy has been concerned with many of these developments in the past and will continue to play an important role with such progress in the future. In addition, it must now assume the leadership in focussing attention on the scientific principles governing man's behavior in his adjustment to a changing environment. The chemical and physical changes resulting in response to physiological stresses due to the impact of environmental conditions and the effect of such changes on emotional and mental states, with the resulting behavior patterns, must be understood and interpreted, so as to help man in shaping his judgments that he may act with wisdom and tolerance.

There is much information scattered among the scientific publications on special bits of evidence from animal experimentation and some from clinical observations of nervous disorders and insanity in man that, when correlated and evaluated, may furnish valuable data which would serve as a guide to future study of the problem. In our anxiety over the physical ills that beset man, we are prone to overlook the mental derangements which are both costly and dangerous for the body politic.

In spite of the fact that a large percentage of all the hospital beds in this country are occupied by mental cases, the continued inhuman treatment of many of these unfortunates reduces them to the status of lost souls. I hope that the Academy will give careful consideration to the sponsoring of a world conference here in New York next year, perhaps at the time of the meeting of the Twelfth International Congress of Chemistry, at which the essential experimental data bearing on this subject may be presented and evaluated.

Much stress has been placed on the contributions of science to the material progress of mankind. However, it is not what a man *has* but what he *is* that determines his behavior in society. No greater truth has ever been spoken than, "man does not live by bread alone." Its significance is ageless and its application universal. Material possessions in themselves have no power to satisfy man's aspirations and keep ever before him that vision of greatness essential to full and considerate living.

The knowledge and the products which result from the achievements of science and technology do not, in themselves, assure fullness of life or prosperity. This depends upon their proper use. They may be employed for the good of society or they may be used as tools and instruments of destruction. What one does with what he has depends upon his sense of spiritual and moral values and not on scientific laws and technological skill. However, science can not take refuge behind the statement that its function is only to provide the knowledge by which the material things of life may be made available. It must share the responsibility with those who use the knowledge for what is done with that which it creates. Maximum human good must result from the interfusion of ideals and practical achievements.

Some critics blame science and technology for most of the ills of modern society, seeing in the products of the scientific age the means for destroying the moral fiber of man and converting him to an irresponsible, selfish, cruel, intolerant being, fostering wars which will ultimately destroy our civilization. This is nonsense. The misuse of the good things of life, regardless of how they are provided, results from certain defects in human character. It stems from selfishness, suspicion, and jealousy, which are not the result of scientific discoveries or the technological developments of these. They have always played tragic roles in the history of mankind.

It is pertinent to inquire if the methods of science could not be applied to the improvement of human relationships. Science seeks facts, arranges them in proper order evaluates the results without prejudice, an interprets their significance in relation the conditions surrounding the proble What catastrophes might be avoided in t future if the scientific method were applie to the solution of international problen

Much has been accomplished by scien in penetrating to the core of the atom an to the heart of the gene. The knowled gained should tend to create a better wor —one in which all peoples can live in peac and harmony. The power derived from atomic energy should enhance social and economic progress, if we would talk less about its devastating potentialities in war and concern ourselves more with its development for peaceful pursuits. The will for peace is the only sure power factor that can prevent war and its consequences.

NEW MEMBERS

Elected May 25, 1950

SUSTAINING MEMBERSHIP

Beherens, Herbert C., Ph.D., Physics and Geology. Chief Engineer, Diebold Incorporated, Canton, Ohio.

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